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MR. ABERNETHY'S

PHYSIOLOGICAL, PATHOLOGICAL, AND SURGICAL OBSERVATIONS;

DELIVERED IN THE

ANATOMICAL COURSE OF LECTURES,

At St. Bartholomew's Hospital.

On the Female Organs of Generation.

(Continued.)

We left off yesterday, by saying, that those birds laying the largest eggs, afforded us the best opportunity of examining the formation of the embryo. Now what do you see in the goose's egg? Why, it is a white speck you behold in it; it is a membrane; it is called the *cicatricula*; the vulgar people call it *the tread of the cock*; but then it is there, though the cock has never trod the hen. I kept pullets in a garret all the winter, and in the spring of the year following, they began to lay eggs; I gave the eggs to *Mr. Clift*, to see if he could find any difference in the cicatricula, but he said he could not; it is a small speck of a whitish colour. Then, upon incubation, this speck enlarges; and it's curious to observe how it is contrived to expose this speck to the warmth of incubation. There are spiral cords to go from either end of the egg, to connect the yolk to it; and put the egg down in any way you please, the cicatricula will be always uppermost, and of course always nearest to the breast of the hen.

Well, this speck enlarges, that's the first thing to be observed; then, another circumstance is, that a zone of dots appears round the circumference of the cicatricula, a zone of bloody points, as Mr. Hunter said. He had a curious sort of whim in his head, for he thought the blood was the first thing that was made, and the vessels afterwards. Now these points enlarge, and eventually seem to connect themselves with a point in the middle of the cicatricula which beats, and which is called the *vesicula pulsans*; the circular vessels turn out to be, those sort of things which serve the embryo as the lungs, and the *vesicula pulsans* eventually turns out to be the heart. What Mr. Hunter says goes to prove this, that various things are formed in various parts of the cicatricula at the same time; that there is no primary fibre from which all the rest proceed; that this is a part endowed with life, and that all parts are formed simultaneously. There are some who think there must be some elementary and primary fibre, and some get that sort of notion into their heads, that they cannot believe there is any thing like formation—that it must be development—that it must be evolution. Now, I say we have no sort of thing like this; I say the power of a female in forming an ovum, is clear, but that, however decidedly it may be formed, the actions will not go on, nor will a young plant or animal be produced, unless this part is stimulated by the semen of the male; if this be done, actions go on, and a young one is formed, resembling both its parents, and that is a most marvellous circumstance; every thing relating to this subject is marvellous.

Well, then, as the process of incubation goes on, you find the vesicula pulsans seems to be contained in a kind of circular or oval surface; this eventually turns out to be the body of the chick. There is another little sort of spiral part, which turns out to be the head, as it proceeds, of the chick; and the wings and limbs seem to grow from it. It is altogether not like evolution—it is like formation.

Well, I have done with that subject; those who are curious in tracing the history of formation of the young, of course attend to this, that there are some animals that are between the oviparous and the viviparous kind. One cannot express the thing plainly; they are oviparous, but the egg is hatched in the body of the parent animal. This is common in the lower class of of animals; and it is even so in reptiles. The viper is an ovoviparous animal, as its name implies; but this is even met with in qua-

drupeds. Mr. Hunter took a great deal of pains to get the ostrich to breed in this country, but he could not. We have become acquainted with a monstrous animal, the *kangaroo*; it's as big as a large sheep; and this great animal produces a young kangaroo that does not weigh above twenty grains. I have seen it when it did not weigh a scruple. It seems to be an ovum hatched in the uterus; and then, when it is hatched, it is excluded from the uterus, and received into a sort of *pouch* at the bottom of the abdomen; then grasping hold of the nipple of its mother, it suckles when it pleases. Here, too, how curious are all the works of Nature! you know, the old kangaroo is an animal with a monstrous pair of legs, with which it can leap the length of this room, and with four paws which only serve it to scratch or feel itself; but how different is the young one; it is a thing with most monstrous fore paws, but scarcely any hind legs at all: with these fore paws it clings to its mother, sucks nourishment, and then having attained to a certain state, it *pops* out of the bag; and this gives almost the first intimation to observe, that the female kangaroo has been in the family way. (Laughter.)

Now, I say, whether you can see it or not, you must admit that all that De Graaf said relative to the power of generation, is true; whether you can see this process or not, as it is ordinarily carried on, certain it is that it is carried on; and sometimes the *ovula Graafiana* will grow to perfection in the ovary. O, plenty of cases are on record to prove this, but it's gratifying to refer to cases which I myself have seen; and the first case I mention, was the case of a woman who died in the Middlesex Hospital, when a *Doctor Croome*, one of the midwifes, was one of the physicians that attended her. The body being opened, the appearance of a sac was perceived with the broad ligament of the womb about it. Here it is; and here is the uterus enlarged. Now then, this being laid open, it was found to contain a full grown child. This was the left ovarium. How can you prove that, you may say? Because it was included in the broad ligament; because the fallopian trumpet was attached to it in the vicinity of the bag. Now if I tell you that Mr. Hunter examined this particularly, and that he was convinced it was the ovary, even if there were no other facts on record, but there are, that would be sufficient. Very well, this is the fecundation of the ovula Graafiana in the ovarium. But suppose the fallopian trumpet should prematurely quit its hold of the ovarium; suppose a kind of ulcerated process takes place, and an opening is made, and this ovula Graafiana gets loose, but that the fallopian trumpet attracts new powers in the uterus, what then? Why this little atom of vital matter endowed with seed, is like a seed thrown loose into the earth; it sends forth roots, attaches itself to the earth, and lives and grows there. Now this is the case in ventral or abdominal fœtation. About the same time that one case occurred in London, another likewise happened: I have a drawing of it here, but it is horribly badly done: a woman who was pregnant, died, and being delivered, she was opened, and the appearance I now represent to you was exhibited: the bowels being drawn aside, a full grown fœtus was found attached by a sort of mock placenta to the mesentery. They did not make out exactly whether there were membranes or not; but here, as in the other case, you will observe the enlargement of the uterus; that goes on simultaneously, and it is a thing curious and deserving of your consideration. As to vital actions arising out of necessity, you may say, that provided the ovula Graafiana gets into the uterus, there is a necessity for a supply of blood, and therefore it enlarges, but without the ovula Graafiana getting into the uterus, the uterus enlarges, and the parts are all removed from one another; there being a child in the uterus, the mamma enlarges, and the secretion of milk begins just when it is wanted, and not before.

Then with respect to the ovarial fœtus; this takes place. I did not bring down the drawing of it with me; it is in the transactions of a society for the promotion of medical and chirurgical knowledge; the drawing and the relation of the case is given by Dr. Clarke, and a very curious case unquestionably it was:—A woman had an enlargement of her abdomen; it was a very firm kind of enlargement; she gradually got faint, and feeble, and she died. The nature of the case was by no means understood, and therefore they opened the body. The abdomen was found distended with coagulable blood; they cleaned away all the blood; they concluded that the woman had had some aneurism of some principal vessel of the abdomen, and that it had burst. They removed all the peritoneal coverings; sought through all the branches of the aortic system, but found nothing of what they had expected to find. They were going to relinquish the prosecution of the examination, ignorant of how this blood had got into this situation, when some one suggested, or thought, that it was reasonable to believe, the woman was newly with child; this excited Dr. Clarke's attention, and he thought, that if this were so, he might at least see the ovula Graafiana; he examined and he found it; he found that the fallopian trumpet had burst, and that that was the source of all this hæmorrhage. I mentioned to you, that the membranes of this part became so

vascular that they underwent erection from desire; but though very vascular, it is impossible to account for this in any other way than this, namely, that there was something exciting irritation in the part. This was an ovarial fœtus, the fœtus being of a size that it could not pass through the ends of the fallopian trumpet, and which was big enough to be distinguished manifestly as an ovum. I myself gave Dr. Clarke a preparation of a fallopian fœtus, which had attained a very considerable size; he was the midwifery lecturer at the hospital at that time, and I thought it was more interesting to him than to me, so I gave him the preparation.

Well, there is no doubt then that the ovum is formed in the ovary, and that it passes through the fallopian trumpet into the uterus: all this is proved by results in the course of nature. Now suppose this ovula Graafiana to have got into the uterus, what takes place? Why, the uterus enlarges; but it is the *fundus uteri* that enlarges, and really three weeks may elapse and scarcely any thing be seen but a swollen and vascular state of the fundus uteri; but after a little time, you see a little membranous bag, with *flocculer fibres*, shooting from every part. This is the first thing you see in the human ovum; and what do you afterwards see if you open it? Why, you see a little speck of matter, which is the rudiments of the fœtus; it is a little speck indeed, and hangs from the bag; however, as the ovum advances to maturity you distinguish certain parts in it. Now Mr. Hunter's plates were considered as masterpieces, in point of engraving, and yet Soemmering seems to have surpassed him; he has found artists that seemed to have surpassed all others.

Now then, I say, you see certain things in the ovum; you see that it is a membranous bag composed of two membranes, the outer one is called the *chorion*, and the inner one is called the *amnion*, containing the fluid in which the child floats. There is another membrane here reflected, that is produced by the uterus; this is exceedingly abundant in the young brute, and is called the *membrana decidua*. When the ovum gets into the uterus, the uterus throws out a sort of fibres, which are produced in every direction; subsequently these fibres become thick, form the placenta, and the whole communication between the ovum and the uterus is effected by the placenta. In one part of the chorion you have the placenta, but at first the attachment seems to be of a more general kind. Well, then, the human ovum is a bag of water, composed of two membranes, the external of which, called the chorion, is evidently vascular; the internal, the amnion, does not appear to be vascular, but you cannot doubt of its vascularity, since it probably secretes the water in which the child floats. Then the uterus throws out a membrane, the processes of which seem to meet processes thrown out by the chorion; but this general attachment by degrees subsides, and an especial attachment is made between the uterus and the ovum by means of the placenta.

Then what is the placenta? O, you know it very well; I need not particularly describe it. If you take a placenta, and inject it with wax in a child, you will find that the umbilical vein spreads its branches all over the placenta, and so do the arteries. The branches of the umbilical vein coming from the placenta, may be said to be the roots of that vein, being the blood which is carried to the child, and probably its nourishment; but the blood returning from the child goes again to the placenta. There do not appear to be communications large enough to let wax further pass; but there is no doubt that there is a communication, for if you inject with subtile injection, and throw it in by the umbilical vein, it returns by the umbilical arteries, and *vice versa*. It is undoubted that there is a connexion between the placenta and the child; that blood goes from the placenta to the child, and that it returns from the child to the placenta again to have some office wrought in it; and it is reasonable to suppose that the placenta serves the child in some such way as the lungs serve the adult subject; that is, that it effects some change essential to vitality.

Now the ground of this opinion? Why there are children born under circumstances in which the placental circulation has been obstructed. The funis umbilicalis has been known to slip down before the head of the child, and then the head of the child pressing down, squeezes the funis umbilicalis against the pelvis, and no circulation goes on; under these circumstances, the child is always born dead. The placenta is not less necessary to the child than the lungs are to the adult.

But this is one view of the matter. The mind is once set upon an inquiry, and it proceeds to ask, how is all this blood got together—where does it come from? Be aware I am only describing one portion of the placenta, and that is the fœtal portion; for there is another placenta to the uterus, the maternal portion. You inject the fœtal part as subtilely, but the uterine part is not injected; nay, the maternal part of the placenta, this uterine part, may be injected from the vessels of the uterus. Throw injection into the uterine arteries, you inject this and make it as red as possible, but the fœtal portion may be taken off from it, and it will not appear as if injection had been

used. I say they are separate; you can inject them separately, and make each as red as the injection you use, without injecting the other; so that there appears to be a distinct circulation between the uteral and the fœtal parts of the placenta; but doubtless the fœtal part is supplied with blood from the uterine part, but how this supply takes place I believe no one knows. In animals the uterine part of the placenta—in many animals the uterine part of the placenta is of considerable substance, and it forms little cavities into which *nodules* belonging to the fœtal part of the placenta enter; but in the human subject, the maternal part is of a very flimsy portion, and you may, if you plunge a pipe into the placenta, where it is attached to the uterus, throw in an injection which will pass into the uterus. O, there is no doubt that there is a communication, but the communication is intricate and yet unexplained.

Now the question is, Why, why is this intricacy? Can any good reason be assigned for it? The answer is, Yes; seemingly a very good reason. If the vessels of the mother had led into the child; if the blood of the mother had directly communicated with the child, the child would have participated in all the illnesses of the mother. Sickly mothers would have always borne sickly children. It is of great consequence that a child should be healthy; and sickly mothers often bear healthy children. The child has an independent life; the mother furnishes it with nourishment, but the functions are so far unconnected with the circulation and general health of the mother, that the child does not suffer by the maladies of the mother, at least in any considerable degree. Mr. Hunter, thinking on this subject, was led at first to suppose, that the diseases of the mother could never be communicated to the child; but he lived long enough to see his error, for infectious diseases are communicated to the child after a certain period. If a mother has *measles*, *hooping-cough*, or *small-pox*, the disease is not communicated to the child till it is about six months old; after that period, the child is liable to participate in the disease. Children generally do participate in such diseases, and premature labour coming on, children have been born covered with small pox; but this does not happen till perhaps the seventh or eighth month. I knew an instance in my own acquaintance of a mother who had the hooping-cough, and who produced a child with the hooping-cough at the time it first breathed.

Well, then, I have told you as much as I believe physiologically need be told, relative to the human ovum. Some have bothered themselves about the use of the waters of the amnion; some have entertained an idea that the child might drink them, and be nourished by them; but this is very absurd, because children are supported by *blood* from the placenta; and children have been born without the power of deglutition, who nevertheless have been well formed and strong. The waters of the amnion exceed by 30 times the young fœtus at first; and in the case of the old fœtus, the bulk of the fœtus exceeds by seven or eight times the waters of the amnion. You cannot imagine any better defence for the delicacy of the young, than that of having it suspended in a bag of water: any action of the uterus cannot injure it; but as the texture of the embryo becomes firm and capable of bearing pressure without being injured, so do the waters of the amnion go away. You know the growth of the ovum is entirely in the fundus uteri for a certain length of time; at about half the period of utero-gestation, the uterus rises up out of the pelvis, at that time do uterine motions take place in the child, and, as the phrase is, the woman *quickens*. Now at this time, the whole of the cervix uteri is perfect, but by degrees the cervix uteri becomes extended, and made part of the bag that contains the fœtus. The obliteration of the cervix uteri, therefore, denotes the progress of pregnancy, and it is not until the ninth month that it is completely evolved, so as to make a part of the bag that contains the ovum.

Now this, no doubt, is very curious, and is the consequence of the upright figure of the human female. If it had happened otherwise, the weight of the ovum would have gravitated against the *os uteri*; as it is, it can never gravitate till the uterus has obtained its full dimensions, and till the cervix uteri has expanded, so as to make part of the general bag that contains the child. The uterus gradually comes up in front of the bowels, and the progress of its ascent is noted; by the eighth month, it has got above the umbilicus, or as high as the umbilicus; it gets higher still, and seems to fill the whole front of the abdomen; but towards the ninth month, a very considerable change takes place in the situation of the uterus. Actions preparatory to parturition take place, which bring the *os internum uteri*, the *os tincæ*, into the very middle of the superior aperture of the pelvis. These are those adjusting actions preparatory for parturition which take place. Then, at the ninth month, more efficient actions of the uterus take place, and the ovum is protruded against the os tincæ; it yields, but yet it is injured, and forced away, so that blood is discharged from it, and it is made gradually to enlarge. There have been some who have puzzled themselves about inquiring why those actions of the uterus should take place at the generally allotted time; and some have

held, that the burthen is kept as long as it can, and therefore that the uterus is glad to get rid of its contents. But it is held only the same time, whether there is a dwarfish fœtus, or a large one; whether there is much or little of the water of the amnion; and, in short, we cannot tell the nature of these things—it is a sort of ordinance of Nature. Then, when the uterus begins to act, it is aided by the expiratory powers; it is first pushed down by that which is free to move, that is, the waters of the amnion; then by the weight of the ovum, and there is carried forward a portion of membrane, which gradually tends to enlarge the os tincæ. Then it is natural to expect that these membranes will burst in their most extended part; but this does not always happen, for sometimes they burst in the uterus, and then the child carries forth a part of the uterus on its head; but generally it is the other way, and then what happens? Why, then, the head passes down; and what further happens, has been explained in these Lectures, as far as physiology is concerned. Well, the child is protruded, and the uterus is contracted until it has expelled the whole of its contents. The child is protruded out of the vagina, and the uterus is contracted so as to hold nothing but the placenta and membrane of the ovum: resting from its labour a little, it resumes it again, and it expels the ovum. Having contracted, so as to expel the ovum, it must have reduced itself to so small a size, that the vessels will be closed, or will pour forth but a very small quantity of blood. There is no hæmorrhage where the uterus is allowed to expel the ovum after what Nature has designed.

Now, really, there is good in studying these powers of nature, which we should always endeavour to imitate, and not interrupt—always endeavour to aid, and not to interrupt. A child does not grope its way into the world, and get through, just as a *chimney-sweeper* gets out of a chimney, (laughter,) by any efforts of his own; it's expelled, and the parts must be perfectly contracted, when it's expelled from the ovum. But suppose a midwife helps the child out, draws it forth, and so on? the uterus containing the placenta and ovum is not contracted; and suppose those become detached? O, then you have uterine hæmorrhage; and an inclination to get away the *secundines*, may induce the midwife, improperly, to pull. I once heard, what I thought was a good story; a medical man was talking to some comrades in his profession, and he told them that which I dare say he would not have told to others: he said, O sirs, I was quite astonished to find, and I was not at all aware of the very little force required for *inverting* the uterus! Now I took it for granted, he had helped the child into the world. You know that the uterus has been inverted, and that the inversion of it has afforded the means of showing the menstrual discharge.

Now, I did not speak of the changes the uterus undergoes, and yet I should do that. When the uterus is enlarged, O, we see the veins very considerably enlarged; and when we cut through them, they look like as if they were composed of cells; we see the arteries of the uterus also very considerably enlarged. The arteries of the womb have a very curious tortuous arrangement; Mr. Hunter calls them the curling arteries of the womb: a very curious tortuous arrangement. And then, on examining the womb, you have appearances, which Mr. Hunter, and others, have believed to be muscular, and some have denied that they are muscular; but no one can doubt the power of forcible contraction of the uterus, who has had his hand in the cavity of it. I need only appeal to the sense of accoucheurs for confirmation on this point. And now, then, I have done with the history of fecundation, and the growth of the ovum, as far as seems to me to be necessary, speaking physiologically.

The *secretion of milk* begins at this time—at the time when the child is able to suck. Now, with respect to the *mammary gland*: it is a gland that one can hardly describe as conglomerate; it is smooth when you cut into it, very firm in its texture, and much intermixed with fat; it is flat where it lies on the peritoneal muscle, and somewhat convex on the other side. It is a gland sending forth pores, and that's what every surgeon should remember: often have I seen a surgeon take away a diseased lump from the breast; then wiping the breast, he has seen a portion of the gland remaining on the woman, and looking white, he has said, O, I see something scirrhous still. Now, a surgeon should never look there to see whether his operation be complete or not; he should divide the substance he has endeavoured to remove, and see whether there are any ramifications of white bands extending further than what he has taken away; see whether he has taken away all the disease, and all the parts in the vicinity of the disease. It is what he has removed that's to be examined, and not the aspect or situation of the bleeding wound. Well, what can we say of the mammary gland? Why, that it is supplied with arteries and veins, as every other part is; that it secretes *milk*, and that the substance is made of *lactiferous tubes*. You may inject those tubes from the *nipple*. The gland seems to be a concretion of lactiferous tubes; those tubes collect together, and make one tube, which ascends up the nipple, and terminates there. Now the *skin*, as it approaches the

nipple, suddenly changes its nature; it becomes of a *greyish* aspect; this is the *areola*; and this dusky skin mounts over the top of the nipple. The gland is, therefore, of lactiferous tubes, of vascular substance, and covered over with skin. But there is so much vascular matter in the nipple, that it is capable of *erection*; and when it is erected, the flow of the milk is allowed, by the tubes being kept straight, and it runs out; but when it is not in a state of erection, there is an obstacle preventing the flowing of the milk. There are controversies about the number of the lactiferous tubes, which, as I believe, can never be decided, because there are only a certain number that become enlarged to give passage to the milk, and others remain in a contracted state. You may put bristles into them; but, as for seeing what are their exact numbers, I think it is more than any individual can determine.

Well, now, such is its structure, and now as to its use: it secretes milk, the sustenance of young animals. And what is milk? It is *serum* and *crassamentum*: but the serum contains a certain quantity of *sugar*. Now this is curious, that the milk, both of young animals and vegetables, should have a *saccharine vegetable* in them. And what is the crassamentum—the curd of the milk? I cannot tell you; it is an insoluble matter; it is something analagous to blood, as I should say. But besides this, there is oil—*pure oil* in milk; and the propensity of this oil to rise, and carry the curd of the milk with it, is what they call *cream*. If you separate the curd from the oil, you will find the oil to be a very pure oil. There are various opinions in France upon this subject; and we have different proportions of sugar, and different stuff, contained in the milk handed to us, which you may search into, if you are desirous of making further inquiry upon this point; but, for my own part, it is sufficient for me to know that it is a very nourishing food for young animals.

There are two animal substances, which I say contain, and must contain the greatest nourishment; that is, *milk* and *egg*. You know a calf drinks nothing but milk, and the milk is converted into blood, flesh, brain, and bone, with most surprising celerity; *ergo*, it must contain the elements of all these subjects; but I believe it requires a calf's stomach to digest it. Milk is the food of young animals; but I don't think the stomachs of old, or of sick persons, can digest it. And out of an egg comes a chick; the egg must, therefore, contain all the rudiments of the materials of that chick. I say, suffice it for me to know, that this is the most nourishing food for young animals.

But, physiologically, I feel much interested as to the function of this mammary gland, for here is a gland where you have an opportunity of seeing how much of glandular secretion depends upon nervous action. It is really very curious that such a complicated fluid as milk can be prepared by this secreting gland, with such surprising celerity: a cow will yield a pailful of milk, and which must be secreted almost as fast as the blood is formed, and is this not wonderful, with respect to what the vital powers are competent to effect. But suppose the cow had had the pet: suppose the calf had been taken away from her; or suppose that somebody had come to milk her she did not like, would she secrete any milk? O, none at all. You might stroke out of the lactiferous tubes that which was in them, but the devil a drop more would you get. Therefore, you see it requires a tranquil state of mind, and a distribution of nervous energy, for the secretion of milk from the secreting fluid; it is not because the blood secretes, but there must be vital action co-operating, in order to produce the secretion. The effect of those vital actions, in *compounding* fluid, as it were, with such celerity, is, indeed, a matter which cannot but excite our surprise, and though it may excite our wonder, it is a fact we cannot deny.

Morbid Anatomy.—I now speak of the morbid anatomy of the female organs of generation; and beginning with the *womb*, I have to say, that there are diseases in the vicinity of the womb, under its peritoneal coat, under its internal lining, in the broad ligaments, and in the substance of the womb too, which are similar to one another.—They are tumours of a peculiar texture, like *scirrhus*. Here is one exposed to show the texture; this is in the cavity of the womb, formed under the internal lining of it; there are firm fibres, and substances deposited in the intervals, so as to give to the whole a firm degree of hardness, not that extreme hardness, however, that cancerous scirrhus possesses. Yet these tumours never ulcerate; they are not at all of a cancerous nature, they merely increase in growth, and seem, as it were, to tease people to death, by the irritation they occasion in the surrounding parts. Here is a preparation in which there is a lot of them in the broad ligament. Some of these tumours are of a looser texture than others; all this Dr. Baillie has remarked. There are something like cells in them, but they all agree in this particular, that they increase in size, have no malignity in their nature, have no tendency to suppurate or ulcer, are inclined simply to increase in bulk, but they are exceedingly distressing as to their consequences. By pressing on the *ovaria*, they create uterine irritation, irregular and pain-

ful menstruation; by pressing on the bladder, they create a frequent desire to void the urine; and so with regard to pressing on the rectum, they produce *tenesmus*, and prevent the discharge of fœtal matter. Persons having these diseases labour under circumstances which are exceedingly perplexing, and it is not till a very advanced period, when the swelling has called for the patient's attention, and the examination of the medical attendant, that the real nature of the disease has been known. There are people who have been hardy enough to poke instruments into them, for bringing about, as they say, a change, and, they also state, successfully too; but this is a practice which I know nothing of. To pass an instrument into the womb, where you are uncertain what you may wound, is certainly a kind of practice that should not be projected, until the beneficial effect of it has been substantiated by the relation of a number of cases, and by the concurrent opinion of the profession. Now when these things, growing beneath the lining of the womb, increase to a certain size, they are protruded out of the *os internum uteri* into the *vagina*. Here is a very large specimen of that sort; and then this is often accounted a polypus of the uterus. They have absolutely become so large, that they have projected out of the *pudendum*, and descended half way down the thigh. I saw a preparation which Dr. Coombe possessed, where the bladder and part of the pelvic viscera were actually drawn a considerable way down. Being sometimes of a pyriform shape, they have been mistaken for an inverted uterus. These are the tumours which you may tie, if you can get a thread or wire over the neck of them; when that is done, the tumour drops off, and the patients are as well as if they had never had any such disease. These are the tumours which Leveret invented a sort of wire to tie; he put the wire round the neck of them, and twisted it daily. There are various modes of tying them. Well, then, these tumours, when they present in the uterus, are esteemed, perhaps, polypi of the uterus; but there are polypous growths from the uterus, and here is a preparation of one, it has a thick base. Here is one of those injected, but still it seems to be a tumour with a thickened base.

What I want to tell you is this, that I know very well, that where there is a tumour carrying down with it a process of the membrane of the womb, there is no doubt that that may be tied, and that no harm will result from it; but even those with thickened bases may also be tied, and no harm result from it. But I have met with many cases in the course of my life, where medical practitioners, tying these *polypi*, as they are called, (for they all go by that name,) with thick necks, have included a portion of the uterus. Those tumours sometimes form within the *parieties* of the uterus, and they may drag down a portion of it; but if the uterus is included in the ligature, generally fatal irritation succeeds. Many times have I been called upon to open the bodies of people who have died after they had had what was called polypi tied; and I have found, that a portion of the uterus was included in the ligature. Now this should make us cautious in our proceedings.

Well, then, so much for those diseases, and now, with regard to the diseases of the womb itself: you find the womb chronically enlarged and diseased; in some cases, the whole substance of the womb is thus situated, and it is known, that in these diseases there is sometimes morbid, and often fœtid secretions, from the linings of the womb. Now this I deem right to mention, because I would not, if I found a case of this kind get well, report it as a case of cure of cancer of the uterus. Here is a specimen showing a uterus chronically diseased; the whole substance of it is thickened. But in the cancer of the uterus, the disease always begins in the cervix uteri: and there are two sets of these diseases; one that begins with scirrhosity, and then goes into a state of ulceration, which is a fatal disease. Here are a number of specimens of them. But the *fundus uteri* remains sound to the last. It begins in scirrhosity in the cervix uteri; and this scirrhosity, which goes into cancerous ulceration, generally begins where there is most mucous secretion, as I may say, about the pylorus, and so on; it extends progressively, and of course the fundus uteri is most remote, and remains sound to the latest period. But there are other cases in which the ulceration in the cervix of the womb throws out a *fungus*—a queer sort of fungus. Here is a specimen of that kind, but it equally shows the fundus uteri, remaining sound, till the disease has so spread as to destroy the life of the patient.

Then I must speak of *morbid appearances*, as circumstances contained in the womb itself: and here I have to mention that there are *hydatids*. Here is a specimen of uterine hydatids; they are of an oval form; they have necks, and they seem to be attached to one another. The uterus becomes distended with them; the weight of extension expels them; and the uterus being enlarged to a considerable size, when they are discharged, there is a great deal of blood discharged from the uterus. I have known many people, as I may say, *delivered* of hydatids, and they have done very well; I have known some, who have been delivered more than once of such a burthen. Ruysch had an opinion, that if there were hyatids, there was an *ovum*; but I know not, if any one has had

an opportunity of making remarks on this subject, because the woman discharged the hydatids, and then we see no more of them. I say that I know of none, but *Sir Everard Home* says that he examined the uterus of a woman distended with hydatids, in a workhouse in this town; and he seems to think as Ruysch did, that there was an ovum, but he has produced no voucher that I can see for this opinion. He says there were membranes, but he only supposes they were the membranes of an ovum. Now, what do we know of hydatids? Why, that whenever they form, they are either in a natural cavity, or in some membranous cyst; so that the finding of a membrane, or a something thrown out from the lining of the uterus resembling a membrane, would not be an appearance like what you would expect where there is an ovum; and it does not identify that that is the membrane of an ovum.

I may here say, that the ovum itself often goes into a diseased state, and thus the child does not grow; that there are diseases of the *placenta*. Here is a specimen of one—of a diseased state of the ovum, such as prohibited nourishment being conveyed to the *fœtus*: the little fœtus is hanging to the morbid ovum; sufficient nourishment has been conveyed to maintain the life of it; but not sufficient to enable it to grow. When people have an ovum of this kind, in a diseased state, after a time it is expelled, and they are delivered of a very strange thing—of a *mole*; as they call it. And a case occurring near this Hospital, of this kind, I went to see it, and I brought away the preparation I now show you, which I found to be the case of a placenta, from its being covered with membranes.

Ovarian disease.—Well, I can say no more relative to the disease of the uterus; and that there are such things told in books, though I have not seen them, as dropsy of the *Fallopian trumpet*. But diseases of the *ovary*, which is the next subject I have to call your attention to, are exceedingly common indeed; and it is curious to know that a little body like the ovary, not so big as a pigeon's egg, will swell to an enormous size. Now, when it enlarges, it generally has cells in it, and those cells are filled with gelatinous fluid. I show you here a portion of an immensely large ovary, which has been injected. Sometimes, however, a single cell fills and enlarges, and grows till it occupies the whole of the abdomen, and then this is what we call a dropsy of the ovarium. Sometimes the whole enlarges together—there's a congeries of cells, and thus all are enlarged, having a kind of sarcomatous structure, and sometimes the case is the compound of these two: one cell enlarges, or two cells enlarge, and you are obliged to tap them on opposite sides of the body; and there is a certain portion still sarcomatous. Now these are the circumstances you may meet with in diseased ovaries. I say, it is curious that a little part like the ovary should acquire such a great magnitude. O, the cells often become immensely large, as I shall tell you. You can do nothing in the way of operation, where the ovary is of this cellular structure which I have exhibited to you; you cannot tap a thing of that sort; if you did, you can let out nothing, and it is very dangerous to do it. I have seen where people thought there was dropsy in the ovarium, that they plunged a *trocar* into some solid substance, but the patient has died. I have known many cases, of solid substance of the ovary, where it has attained a certain magnitude and then ceased to grow. I have therefore to declare to you, as my opinion, that where patients have a filling up of one cell of the ovary, or of many cells of the ovary, that the case ought to be treated upon purely surgical principles: that when you find the body rising up on one or other side, and painful, you should keep your patients in a horizontal situation; apply leeches, blisters, and produce irritation, for I can assure you that I have seen many enlarged ovaries arrested by this treatment. Of course you would attend to the state of the patient's general health, as I imagine you would do in every case, in which a patient was affected; but I say this should be treated upon surgical principles, just as you would an enlargement of any other part of the body.

When the belly is distended, and where there is one great cyst, forming the principal bulk, the question is, about tapping it. Now I should say to you, you must tap where it becomes inconveniently large to the patient. It is a cruel thing for a female to be subject to periodical tappings, but there is no help for it. I shall tell you cases of postponement of this operation, in order to show you the consequence of such postponement. It is a curious thing too, that sometimes when the ovary increases in this manner, the urinary secretion is suppressed. This is one of the circumstances that probably has led to the treatment of the disease, as if it were a dropsical disease, as it is called—a dropsy of the ovarium. It is not in every case there is this suppression of the urine, or rather diminution, for it is not an actual suppression, but a *diminution* of the quantity; but where such an occurrence takes place, it has been observed that the secretion of urine becomes renewed, and the fluid of the ovary is dissipated. A lady who had had this happen to her several times, went on in hopes that it would again occur, till her ovary became most immensely enlarged indeed, and then she was obliged to consent to the dreaded operation of tap-

ping—dreaded by her; and I let out from the ovary a washing-tub full of fluid, a pail, and many wash-hand basins full. I did not measure the quantity, but it was very great indeed. I closed the aperture, and bandaged her up. Now the consequence? Her abdominal muscles had been so overstretched, that she could make no use of them; she could not cough: the following night she had some irritation in her throat, and she wished to cough, but she was unable to do it; she poked her finger down her throat, with a view to assist in removing the irritation, but this only made bad worse, and her sufferings during the night were almost intolerable. This is overstraining the abdominal muscles. Now another case: A young lady who was really exceedingly vain of her fine form, had it spoiled by dropsy of the ovary. She was so annoyed by this, that she secluded herself from the view of every one; she went into the country and left her family; she became very large in the abdomen indeed; she was miserable, and altogether it produced a great decline in her health; but she shrunk from this operation of tapping, till at last she was reduced to extremities, and was brought back to the residence of her parents. A physician was sent for; he had seen her in the early stage of the disease; he said it was a diseased ovary; he said it must be tapped; and I was sent for to perform the operation, but I could feel nothing like undulation, and I said, I don't think there is fluid in it. O, said he, that's quite nonsense; you must perform the operation; I have known the case from the beginning, and why should you be afraid to perform the operation? I said, it may be a sarcomatous ovary, but if you demand it, I am ready to introduce a trocar, and I did. What were the contents? It had been jelly; it was now almost a solid substance, but there did ooze out a sort of thing that would scarcely pass through the aperture, which the *Doctor* took hold of, and dragged out to a considerable length; but really there was no good done by the operation. Well, you must go on with the practice of your profession, according to those rules that experience has established to be right. But this is so deplorable a thing for a person, apparently otherwise healthy, to submit to this periodical tapping, that almost every medical man has been devising means to cure the disease, or to prevent the necessity of a recurrence to this operation. Now we are not the arbitrators of the life and death of people; if they die in consequence of disease, why it cannot be helped, and people submit to it, for they know it is inevitable; but we had better not be making experiments that probably may destroy them, and bring disgrace on our profession. What has been done? Why, people have passed setons, have thrown in injections into diseased ovaries, and so on. I have known many of these pranks played in the course of my life time, and they have all been fatal. Certainly, in common sense, no one would treat a diseased ovary, as they would do the common *hydrocele* of the *tunica vaginalis*. *Le Dran* did puncture the ovary, so as to have a fistulous opening, out of which the water could be passed; but I don't know that the patient's state was much bettered by that. I have seen this, however, practised, where there were divers cysts in one ovary, and a good deal of sarcomatous swelling, in order to prevent the repetition of the tapping. The lady whose case I have mentioned, said this to me, "What, must I submit to this periodically? Is there no mode that can be adopted to obviate such a course?" And a very rational lady she was. I told her that there was no mode, save this one, namely, putting in a *canula*, and leaving it in, but that I thought that as bad a doom as the other. She said, "I shall have that done." She was a woman of great determination, and of great intellect; she decided that she would have it done, and that if I did not choose to do it, some other body should. Well, then, I tapped the abdomen the second time, or rather the third or fourth time, for it had been tapped several times before, and put in through the canula of the trocar a catheter, and fixing the tube, I dressed the part up, and put on a bandage. I thought this was a very good way of proceeding, but the case terminated fatally. Now I know Dr. Gooch was much inclined to recommend that operation; I have only, however, to tell you what I know myself respecting these cases; and I say, I should be more disposed to let disease destroy the patients, than to incur the blame of having at all aided in their destruction.

I omitted to say this—I am convinced that an enlarged ovary, treated on surgical principles, even subsequent to these operations, may do good. And here I have to tell you what's come within my own knowledge in treating the disease in this manner. I have tapped many ovaries, and by producing counter irritation with blisters and so on, I have kept them from filling again. I could tell you a very considerable number of cases, but the case of one old woman shall suffice. This old woman I kept nine months in the hospital, and there was no filling of the ovary; I could not detain her any longer; it would not have been right to have done so, and I let her go; but I said to her, if ever this should fill again, do apply early, and she said she would. However, I saw not the old woman again for two years, and then she was a patient in the house, with a sloughing sore of her leg, and her abdomen

was full. I asked the old woman why she did not come early, according to her promise; she said she did not like to be blistered. She told me, she had not filled for a year after leaving this hospital; that she had gone to a dispensary, and got tapped, and that this was the second time of filling since she had been in this hospital before.

Now the diminution of urine is a curious thing. Some would be inclined to say it was the pressure of the ovary upon the kidneys that caused it; but that opinion cannot be maintained, for the ovary will fill, and the secretion will come on when the ovary is at its greatest magnitude; just as I have observed in the case I have mentioned, for several times that lady was relieved in this way, and which induced her to postpone the tapping.

I remember, when I was quite a boy, hearing this case: that an old woman, or an oldish woman, with an exceedingly large belly, was waddling over London Bridge on a cattle day, and there arose behind her a cry of mad ox, mad ox! She was terribly afraid; she waddled into one of the recesses on the bridge; the ox passed her; and from the state of fear into which she was thrown, she immediately began to piddle, and the water continued to flow from her, until she was as light as a greyhound. (Laughter.)

Now this suppression or diminution of urine cannot be owing to pressure; and what is it owing to? O, no doubt, it is a *sympathy*; no doubt that the kidneys are sympathetically affected with the ovary. I would not tell this to those who choose to be incredulous, but I tell you it, as my opinion of the only solution of it; and if you can keep the ovary exempt from irritation, you will keep the kidneys from this sympathetic affection. Thus it has happened when I have treated the disease of the ovary, by surgical measures, after the depletion of their contents, that I have prolonged the secretion of urine for an indefinite period. I do not remember whether, in the case of the lady I have mentioned to you, there was a diminution of urine; I do not remember whether there was or not, but I remember many patients in this hospital in whom it was; one woman in particular—an old *stager*, who used to go into all the hospitals in London, to get tapped; I tapped that woman; afterwards, I put on the bandages, leaving a little sort of trap-door, where I could apply a blister; by doing so, keeping her in a recumbent position, and attending to the state of her bowels, she has got round, but her urinary secretion used to stop on the third day.

I may tell you, that whenever the water is discharged, in general there is a flow of urine at first—a considerable flow of urine from the relaxation induced in the cyst of the ovary; but, on the third day, the ovary begins to fill again, and the urinary secretion is diminished. It is not in every case of diseased ovary you find this; sometimes you find it, and sometimes you do not; and this is the case of all sympathetic affections that I know anything of: the stomach and the head sympathise; and there are people, too, of diseased stomachs, without cerebral affection.

I have a preparation here of the state of an ovary, where, instead of jelly, or a sort of gelatinous fluid, or a bloody fluid being found, there was stuff like water-gruel in it; this stuff became solid from residence, and was afterwards rolled up into balls like gun-bullets. The surgeon who examined the body, and who gave me the preparation, was much surprised at this form of appearance. There was also a great ball of *hair* rolled up, and plastered over with this water-gruel sort of stuff.

Yes, you find hair grow from the sides of the ovaries, and afterwards become detached; but it is curious that this hair has no bulb to it, as hairs in common have. Also oil is found in these cysts; oil which becomes like suet when cold. If you open the body when warm, you will find it to be oil, but if you allow the body to get cold, this becomes of a suety sort of consistency.

Also in the ovary, you sometimes meet with *bones*; and whenever bones have been met with, the medical man who has examined the case, has been apt to suspect that there was a fœtus. This was the opinion formerly, that there was an ovarial fœtus in such a case, and that the rest of the fœtus had decayed and been absorbed, but that these bones had been left: now Doctor Baillie invalidated this opinion, by the examination of a child who must have been a virgin, and in whose ovary he found a bone. Then I dare say those who are disposed to speculate, might say, O, well, I don't wonder at that; the ovary is kept down with very peculiar and elastic powers; it may form bone; it is intended to form bone, which might be the rudiments of the embryo, and so on. Now that does not at all seem to me to be sound physiology; and it is invalidated by this case—a case which occurred in an animal that was neither male nor female, *to wit*, a *gelding*. (Laughter.) One of the horses of the artillery at Woolwich died; Mr. Coleman had him opened, and he found below his kidney a kind of cyst which had in it a tumour, of a fleshy hardness; he tried to cut it through, but he could not, and he was obliged to saw it. It was a fleshy tumour in a sort of cyst, but containing *horse-teeth*—large horse-teeth, with processes of enamel descending be-

tween the bones of the teeth. Now these could not have been formed, but upon the bulbs, and upon compound capsules. Well, then, the vessels of a part seem competent to do anything, and to build up any kind of structure. Now this is what I have to say about the disease of the ovary.

Superfœtation.—A great deal of fuss has been made of late where appearances have been met with, that have led to the idea of the existence of what is called *superfœtation*, as if there was an embryo within an embryo. You have heard of those cases, doubtless. A person has some swelling in the abdomen, the nature of which is unintelligible; and, subsequently, the patient dying, on being examined, it is found that this swelling is something like a fœtus; that there is a head, and a trunk, and limbs to it. But, in general, nothing has been made out, as to the supply of such a fœtus. I have no doubt it is supplied, if the case was made out at all, by the *aortic system* of the subject. But it is supposed to be a sort of second conception—a fœtus within a fœtus, and looked upon as exceedingly marvellous. Now, if the same thing had grown upon the *back* of a patient, why there would have been no marvel in it: do we not find children formed with superfluous membranes, and yet consider them as no matter of surprise at all? I may be wrong, you see, in these opinions, but I tell you my notions about them. To me there does not seem to be anything very remarkable.

Monstrosities.—I have not talked of *monstrosities*; the subject is endless; but I wish to tell you, what seems to me, physiologically speaking, the most curious thing with respect to monstrosities, as related in a case by Dr. Clarke: a woman was delivered, who had two ova; out of one ovum was formed a healthy well-formed child, but there seemed to be more in the uterus; and out of the other ovum there was delivered a mass of flesh, about seven or eight inches long, three or four inches in breadth, and one and a half inch, or two inches, in thickness; in one part of it there was inserted the *funis*, and the other end was attached to the ovum; it had neither head nor extremities. Dr. Clarke took it home, and injected it from the funis; he made it red throughout with injection; it was a fleshy mass; it was cut asunder, and what do you think he found in it? Heart, lungs, or digestive organs? Not one of them; it was a vascular mass throughout; but nearly extending the whole length of it, there was a gigantic thigh-bone, much bigger than could have belonged to any child born in the natural way. You will say, what vessels were there to supply this bone, when there was neither heart nor anything of that kind found in the mass? Did the vessels emanate from the placenta which produced a mass of flesh, and a well modelled thigh-bone, differing in nothing from a natural thigh-bone, saving its gigantic size?

Well, I say, in a physiological point of view, this is curious. In general it is said, where products come out from a second ovum, that there is a communication between the vessels of the placenta of the two ovums, so that this accounts for a sort of mode of conveying nourishment.

Well, this is all I have to say on the subject of the morbid anatomy, or its appearances in the examination of the female genitals.

LECTURES

ON THE

Diseases of the Nervous System,

BY

DR. CLUTTERBUCK.

Lecture XII.

On Specific Fevers.

This name is given to a class of diseases which have all the essential characters of *idiopathic fever*, but with several peculiarities by which they are distinguished both from ordinary fever, and from one another. They have been usually termed *exanthemata*, or *eruptive fevers*, from their being commonly attended with an efflorescence or eruption of some kind on the skin, different in the different species, as I shall presently describe to you. There is a considerable number of these *specific* or *eruptive* fevers, the chief of which are *small-pox*, *chicken-pox*, *measles*, *scarlet fever*, the *vaccine*, *cynanche parotidea*, or the *mumps*, and probably others less known. These are all sufficiently characterised as *specific fevers*; but others have been included in the class, with doubtful propriety. Such are *pestis* or the *plague*, *erysipelas*, *miliaria*, *urticaria*, *pemphigus*, and *aphtha*. I shall proceed now to explain to you in what respects these *specific fevers* differ from ordinary *idiopathic* fever, and then describe the peculiarities of each.

The *specific* fevers commence with the symptoms of ordinary *idiopathic* fever, both *local* and *general*. The manner of attack is the same. There are, for example, the *local* symptoms of pain in the head, with disturbance of the *sensorial* functions; and the same *constitutional* disorder, (*pyrexia* or a febrile state of system). So that, at first, you would be unable to ascertain the precise na-

ture of the disease. If, for instance, you saw a patient during the eruptive fever of *small pox*, and before the eruption had made its appearance on the skin, you would be unable to determine the actual nature of the disease present. You would say, the patient is labouring under *fever*; but nothing further. It is not till you had watched the progress of the disease for a certain time, that you would be able to determine its particular nature.

The peculiarities of this class of diseases are various.

1. They are *contagious*, in the strictest sense of the term, as I before explained to you: that is, they are produced, each of them, by the action of a peculiar poison or *virus*; which *virus* is generated in the body of the sick while labouring under the disease, and communicated to another, either in the way of actual contact in a solid or fluid form, or by effluvia. There is no sufficient reason to believe, that they ever arise spontaneously, or from any other source than that I have just mentioned.

2. They are all of them, comparatively speaking, *new* diseases, no mention of them being made in the works of the older writers; such as those of Hippocrates, Aretæus, Galen, Celsus, or others equally remote. The exact time of their first appearance is unknown; nor do we know any thing certain regarding their origin, so as to enable us to account for their existence at one time or place, rather than another. It is not improbable that they may have had their source in the application of a *virus* from some animal in a diseased state; and the disease, when thus once excited, might have been transmitted afterwards from one human subject to another; just as we see to be the case with regard to the *vaccine* disease. But this is mere conjecture.

3. They take place but once in the same individual, those who have gone through the disease being afterwards exempt from their influence. This rule is general with regard to all of them; though there are probably exceptions. These, however, are very few; and indeed most of those that have been adduced as such, are equivocal.

4. They have peculiar characters, and their progress and termination are likewise peculiar; by the observation of which they may be distinguished from other diseases, and from one another.

5. They are for the most part incurable by art, having a determined course to run, when they terminate spontaneously, and for the most part in health. This course can hardly be prevented, nor materially interrupted without disadvantage. All that, in general, art can do towards their relief, is in the way of mitigation merely.

I shall now proceed to mention them individually, and nearly in the order given above. It is not necessary to be very minute here, because you will easily perceive that the treatment is very simple, being merely *palliative*; while the means we employ are of the simplest kind; for we have no *specific remedies*, such as *mercury* is in *syphilis*.

1. *Small pox* (*variola*). This is the most important of the *specific fevers*, for none, probably, has been so destructive to the human race. The first mention made of this disease is in the writings of the Arabian physicians, about the fourth century; but it is not spoken of as a new disease, but as one with which they had been long familiar: nor is any allusion made to its origin or first appearance.

The attack of the *variolous fever* is said to be distinguished by an unusual degree of pain in the back and loins, and also pain upon pressure on the *epigastrium*. These symptoms, though often present, are not at all times remarkable. About the third day from the commencement of the fever, there appear on the skin very minute red spots, at first scarcely larger than flea-bites, and without perceptible elevation. In a few hours they spread, become elevated, and gradually rise into pimples, which go into suppuration, in the space of from four or five to eight days or longer, depending much upon the number of the pustules present. The pustules thus formed, gradually become dry, and in general without breaking, the fluid they contain being either dissipated by exhalation, or absorbed. A brownish crust forms on the surface, which, in a week or more, falls off, leaving a red spot behind on the skin; and often a depression or *pitting*, which remains for life. It is commonly many weeks before the skin recovers its natural hue.

There are many varieties to be observed in *small-pox*, both as regards the *fever* and the *eruption*. Thus, the *eruptive fever* is at times so mild, as hardly to be noticed; and in this case the succeeding eruption is trifling, in point of numbers: in such cases, the disease is likely to be overlooked, and hence, probably, many are said to be insusceptible of the disease, who have already undergone it in the mild way I have just described. At other times, the fever attacks with great violence, and in infants is often attended with convulsions; but this is not necessarily followed by a copious eruption, often indeed the reverse: but in general there is a proportion observed between the degree of fever, and the eruption that succeeds. If the eruption is not very abundant, the fever commonly subsides on the eruption being completed: but if the pustules are very numerous, the fever con-

times; and this is always an unfavourable sign.

There is great diversity also, in respect to the eruption itself, which is not only more or less numerous, but with very different characters: 1. There is the *distinct kind* (*variola discreta*), where the pustules are more or less numerous; but, however numerous, are nevertheless accurately circumscribed, and distinct from each other: in this case, the fever generally subsides as soon as the eruption has taken place. The character of the pustules differs likewise in other respects: sometimes they are white, and contain a puriform fluid; at other times, they are transparent, and contain a limpid fluid (*crystallina*). Sometimes, again, they contain little or no fluid (the *warty pock*). All these are of a mild nature, and rarely dangerous.

2. The *confluent small-pox*, where the pustules are extremely numerous, with an irregular outline, flattish on the surface, and breaking into one another. In this case, the fever continues throughout the whole course of the disease, which is always then attended with much danger. Occasionally, *purple spots* appear in the interstices of the pustules, and the fever assumes a character of great *malignity*.

In the *distinct* sort, if the eruption is numerous, the febrile symptoms are renewed about the period of suppuration; this is called the *secondary fever*.

The pustules appear first on the face, then on the trunk, and lastly on the extremities; there being a day or more of difference in the time of appearance: and they follow the same order in their progress to suppuration, or *maturation*, as it is usually termed. This process is completed in seven or eight days in the mild and *distinct* sort; but where the pustules are very numerous, a day or two longer is required. When the disease is *confluent*, it is commonly much longer, as twenty days or more, before the whole have maturated.

Prognosis. When *small-pox* takes place casually or *naturally*, as it is called, and not by inoculation, it proves fatal in a large proportion; the mortality has been variously stated: as by some, as high as 1 in 3 or 4. In hot climates, and in peculiarly unfavourable circumstances, this may probably be the case. In favourable circumstances, it is probably not as high as 1 in 10. This, however, you must allow, is still a large proportion; making it a matter of great importance to mitigate by art, or, if possible, to *prevent* the disease altogether. How far this can be done, we shall presently consider.

The danger of *small-pox* depends partly upon the number of the pustules, partly upon their character, and that of the attending disorder of system. A very numerous eruption is always attended with danger, from the mere extent of inflammation present. The *confluent sort* is still more dangerous: and the same is the case where *purple spots* appear in the interstices, with other symptoms of *malignant* fever. *Small-pox* is most dangerous in persons of high health; in very young infants, who are often carried off by convulsions; and also, in old persons. It is dangerous to women in the advanced stage of pregnancy; and in the earlier stages, is very likely to produce abortion. The time of greatest danger is from the 11th to the 20th day, and before the maturation is completed. The signs immediately indicating danger, are a sudden shrinking of the pustules, with coldness of the extremities, succeeded by great restlessness and delirium, and, at length, stupor.

Treatment. I have already told you that *small-pox*, like the rest of the *specific fevers*, is incurable by art: the disease will run its course, in spite of remedies. Your object should be, therefore, to mitigate its violence, not to attempt to interrupt its progress altogether; for you thereby only make it irregular, and protracted in its course, with a risk of rendering it more dangerous. It is a highly inflammatory disease, and requires *antiphlogistic* treatment, especially in the early stages, and indeed as long as the pulse retains its strength and fulness. The propriety of *blood-letting* to any great extent, is, however, questionable. Those of the greatest experience in the disease, are against its use, considering that it may do harm by disturbing the natural and necessary course of the disease, so as to increase its danger by impeding the process of maturation, which appears to be essential to recovery. In general, mild cathartics, with the admission of cool air, are all that is required. It was Sydenham's practice to give an *opiate* at night, throughout the disease; and the had great experience of it in its worst forms. Our present experience of small-pox is too limited, to warrant my giving you a confident opinion on the subject. Towards the end of the disease, when the skin turns pale, and the pustules shrink, with the restlessness I before described, I believe the common practice of giving brandy to the patient, as well as other active *stimulants*, to be useful. The *hot bath* is also proper on such occasions, used from time to time, but not so long as to induce faintness.

Small-pox is wonderfully mitigated in its violence and danger, by the practice of *inoculation*; that is, by inserting the variolous

virus into a wound in the skin. When this operation is practised under the most favourable circumstances, the deaths from *small-pox* do not amount to more than one in many hundreds; as was proved by the practice of the *Small-pox Hospital*, previous to the introduction of *vaccination*. The *Vaccine Board*, it is true, in their annual *Reports* to Parliament, state the matter very differently; exaggerating enormously, the mortality *of small-pox*, both the *casual* and the *inoculated*. But then you are to consider, that the *Vaccine Board* consists of the heads of the two colleges, who have few or no opportunities individually, from their own experience, of knowing any thing of the one practice or the other: and further, that they divide among them the *lion's share* of the *three thousand pounds* annually granted by a well-meaning parliament, for the propagation of the *vaccine* practice.

No satisfactory reason can be given for the effect of *inoculation* in so remarkably mitigating *small-pox*. The patient, after inoculation, usually sickens about the eighth or ninth day; whereas the disease, when taken in the *casual* way, does not make its appearance till after fourteen days. There is a great advantage in *preparing* the patient for inoculation; and this preparation consists merely in the adoption of an *antiphlogistic* regimen, with the use of a few purgatives, before the time of sickening. It is a great mistake to suppose, as some do, that a state of high health is the most favourable to excite the disease in; the contrary is, in fact, the case: for the same reason that the greater surgical operations are found to be most dangerous in healthy and robust subjects.

The *small-pox* is not merely mitigated, but *prevented*, by the practice of *vaccination*, newly introduced among us. By this practice, a peculiar form of disease is excited, consisting, like the other *specific fevers*, of a *local* affection, and a *constitutional* disorder having all the essential characters of *idiopathic fever*. This fever is sometimes so slight, as to be hardly perceptible: at others, it is very severe; and in infants, sometimes attended with convulsions; just as happens in *small pox*. The *vaccine* disease does not appear to spread by effluvia, but only by the direct application of the *virus* to the skin.

The power of the *vaccine disease* to prevent *small pox*, is sufficiently ascertained in a general way; but, at the same time, the exceptions are so numerous as to detract materially from the value of the *vaccine* practice. Unfortunately, the degree of security afforded against future *small pox*, cannot at present be ascertained; so as to enable us to form a satisfactory estimate of the value of the *vaccine practice*, in comparison with the *variolous* inoculation. The forced, and almost exclusive, use of the *vaccine inoculation*, from its first introduction, has made it impossible to know who are, and who are not still susceptible of *small pox*; for persons are not now, as formerly, much exposed to the contagion of this. Many years must yet elapse, before the true value of vaccination can be determined. The superiority of the new practice is founded on the following points. 1. The *vaccine* disease is far milder, than the *variolous*, as produced by inoculation. 2. It leaves no deformity behind it, as the *variolous inoculation* sometimes does. 3. It is better as regards the public; as it is not propagated by effluvia as *small pox* is, the ravages of which are thus liable to be increased by the *variolous* inoculation, unless practised under particular restrictions. These advantages, great as they undoubtedly are, are counter-balanced in some degree by the insecurity of the *vaccine practice*, which does not, by any means, afford equal exemption from future *small pox*, with the *variolous* disease itself. On which side the scale turns upon the whole, is at present matter of opinion. I think the public should be allowed to judge for themselves in adopting one or the other practice; and which they are as competent to do, and quite as likely to come to a judicious and disinterested conclusion, as the *Vaccine Board* itself. I may observe, in the mean time, that the danger of the *variolous inoculation* has been most unreasonably exaggerated; as you will find on comparing the statements recently made on the subject with the recorded experience of the Suttons, Baron Dimsdale, and the practice of the Small-pox Hospital.

Varicella, or *chicken-pox*, has so strong a resemblance to *small-pox*, as, at times, to be easily mistaken for it. Severe *chicken-pox* approaches very near to *small-pox* in most respects; and the two, no doubt, have often been confounded together. The chief discriminating characters are, the milder nature and shorter duration of the *eruptive* fever in *varicella*—the crystalline appearance of the pustules; and their more speedy maturation and desquamation. As to the treatment of *chicken-pox*, the mildest antiphlogistic means are all that are required.

Rubeola, the *measles*; called also, *morbilli*. This disease, like *small-pox*, is contagious, and spreads widely by its effluvia. The eruption usually appears about the fourth day after the attack of fever. The eruption consists of minute red spots, not of the brightest hue, slightly raised above the general level of the skin, and mostly with an exceedingly small vesicle on the top.

After three or four days, the eruption begins to fade, leaving a scaly roughness on the skin for some time. The fever does not so generally terminate with the eruption as in small-pox, but often continues afterwards, and sometimes in an aggravated degree.

The disease commences with catarrhal symptoms, namely, sneezing, red and watery eyes, and a short, dry, hoarse cough. These symptoms continue for some time after the eruption has disappeared; and frequently the inflammation at this period extends to the substance of the lungs, giving rise to difficulty of breathing, with pain in the chest: a foundation is thus often laid for future *phthisis*. Sometimes the inflammation is concentrated about the larynx, and produces symptoms of *croup*.

The *measles* differ much, in point of violence and danger, in different seasons; sometimes being mild in character; at other times, fatal to great numbers. The disease sometimes proves fatal from the violence of the fever, or *brain-affection*; sometimes from the *pulmonic* inflammation; or from the occurrence of *croup*, as I have just stated. The continuance of inflammation in the chest, in a chronic form, is another source of danger, that ought to be carefully guarded against.

Treatment.—Mild cases of *measles* require only careful nursing, and an antiphlogistic regimen, while the disease is running through its course. If either the symptoms of fever, (*brain-affection,*) or those of inflammation in the chest, are unusually violent, a more active antiphlogistic treatment is required; *bloodletting* more particularly. This evacuation is often more requisite at the decline of the disease than at an early period, on account of the pulmonary symptoms. The antiphlogistic treatment should be continued, and the patient carefully attended to, as long as any signs of pulmonic or catarrhal inflammation remain.

The *measles* are said to be attended on some occasions with *putrid* or *malignant* symptoms, such as *purple spots* in the skin, and the like; but having no experience of this myself, I can give you no directions in regard to the treatment; further than saying, that the best mode of treatment in such cases is, I believe, not sufficiently ascertained.

Scarlatina, or *scarlet-fever*, so called from the bright red hue of the skin. This is a highly contagious disease. The efflorescence usually makes its appearance three or four days after the commencement of the fever. It is more or less generally diffused over the skin, sometimes pretty uniformly, sometimes in patches; there is also, generally, a degree of roughness of the skin, occasioned by extremely minute pimples or vesicles. After three or four days, the efflorescence fades and gradually disappears, the cuticle falling off in minute branny scales; but where the cuticle is thick, as about the hands and feet, it often peels off in large flakes.

In most cases of this disease, if not in all, there is soreness of the throat, arising from inflammation of the mucous membrane lining the fauces; whence the terms *scarlatina anginosa*, *cynanche scarlatina*, and *scarlet fever with sore throat*, that have been applied to this disease.

There is no disease that differs more at different times, in point of severity and danger, than this; and that both with regard to the local symptoms, and the attending fever. In some seasons, it spreads widely as an epidemic, and proves fatal to great numbers; and that more quickly than is the case in almost any other disease, in this climate at least. At other times, it takes place *sporadically*, that is, individually, with little disposition to spread by infection; or if it does so, it is of an uniformly mild character.

In many cases, the inflammation in the fauces is trifling and superficial, or, at most, is attended with slight ulcerations that quickly heal; in this case, the attending fever is also slight and inconsiderable. In some instances, the affection of the throat has not been noticed at all; when the term *scarlatina simplex* has been applied to it. At other times, where the throat has been affected, there has been little or no efflorescence on the skin. Sometimes, again, the inflammation in the fauces has assumed a bad character, ending in foul and putrid sloughs, with deep ulcers; and accompanied with the symptoms of *malignant* fever, that is, a disturbed and oppressed state of brain, the consequence of active inflammation in this organ. These cases prove fatal in a large proportion. To this form of the disease, the terms *putrid*, *malignant*, and *ulcerated* sore throat, have been applied, (*cynanche maligna.*) It has been made a question, whether the *scarlatina simplex*, and the *cynanche maligna*, are not *specifically* different diseases, each proceeding from its proper *virus* or contagion. This point may be considered as still undecided. But seeing that when these diseases prevail epidemically, there are often found, even in the same house, instances of the most *mild* and of the most *malignant* species, I am rather inclined to the opinion that they are only different degrees of the same thing, modified by season, weather, and individual constitution.

In the *treatment* of this disease, you are to bear in mind its *specific* nature, and that it is scarcely curable by art, but will run its course in spite of any remedies. *Palliation*,

therefore, is the great object of practice. Slight cases may be safely enough left to themselves: the more severe ones call for more attention. In the early stages of the disease, the general febrile action is to be moderated by cool air, and keeping the skin cool, by occasionally moistening it with simple water, which is better than vinegar and water. And the regimen altogether should be mildly antiphlogistic. The utility of *bloodletting* is an unsettled point, and one about which directly opposite opinions are entertained. It appears to me to be in general unnecessary; while, if used, it cannot be expected to put a stop to the disease. Were any very urgent symptom to arise, such as early delirium, or croup, and this in a habit of tolerable strenth, I should have no doubt of the propriety of moderate *bleeding*. When *malignant* symptoms present themselves, such as foul and putrid sloughs in the fauces, with a weak and rapid pulse, *bloodletting* would seem to be out of the question. But there is still a doubt as to the utility of *bark* and wine, and *ammonia*, and the like, in such circumstances. The late Dr. Fothergill strongly advocated the use of these remedies, and the practice became in consequence very general. But Dr. Fothergill subsequently changed his opinions with regard to them, and at present they are but little used. According to my observation, such remedies, carried to any great extent, do harm, by aggravating the febrile symptoms altogether. It has appeared to me, that even in the most formidable circumstances, a passive mode of treatment is the best, such as I have mentioned above, as applicable to the early stage of the disease. *Liquid chlorine*, formerly called the *oxymuriatic acid*, was at one time reckoned almost a *specific* in this disease; and large doses of the *capsicum* have also been recommended, both *locally*, as a gargle, and taken into the stomach; but these have been used upon theoretical grounds, rather than from actual experience. I can only say with respect to them, that I have seen no reason to consider them as of much importance.

The *mumps* (*cynanche parotidea*). This is a fever of a peculiar, and evidently contagious nature. The distinctive character is a painful and extensive swelling of the parotid and maxillary glands; which continues three or four days, and then declines; the fever usually going off at the same time. It is a remarkable circumstance, that as the swelling about the throat subsides, an inflammation of the *testes* frequently arises in the form of what is called *hernia humoralis*, but which soon again disappears: at times, brain-affection with delirium follows this subsidence of the swelling of the *testes*. It is said that, in females, the *mammæ* become inflamed and painful, like the *testes* in the male subject: and I think I have seen two or three instances of this.

In general, the disease altogether is mild in its character, and requires only a mild antiphlogistic treatment. If the pain or fever are unusually great, *blood-letting* may be called for, to relieve them.

Urticaria or *nettle rash*, called also *cuero*, has been sometimes classed with the *exanthemata* or *specific* fevers: as has likewise *erysipelas*, and even *aphtha miliaria*, or the *miliary fever*, (so called from the skin being covered with minute whitish pimples, resembling millet-seed in appearance,) has been looked upon in the same light; but there is a doubt with respect to all of these, as to their *specific* character, and as to their being contagious or otherwise. *Pemphigus* is of so rare occurrence, that I can hardly venture to give an opinion upon it. *Hydrophobia* might, I think, with much propriety, be placed among *specific* fevers, for it has the essential characters of fever, together with a peculiar train of symptoms that distinguish it from all other diseases; while, like the other specific fevers, it arises solely from the application of a peculiar *virus*, generated in the body of a rabid animal. Unfortunately, the fatal virulence of this poison is such, as hitherto to have defied the power of art to subdue its effects: and *prevention* alone can be looked to; the only means of accomplishing which, that are deserving of any confidence, is the complete excision of the bitten part. This would be justifiable at any period prior to the appearance of the symptoms. The destruction of the part by *caustic* is less to be relied upon, as it appears to have often failed to prevent the occurrence of the symptoms.

I shall next have to treat of the *non-febrile* diseases of the brain, embracing the class of *neuroses* of Dr. Cullen, the *nervous affections* of other writers.

EXOSTOSIS OF THE LOWER JAW.

On Sunday last, at the Hospital of Surgery in Panton-square, Mr. Wardrop, assisted by Mr. Bennett, removed *rather more than half* of the inferior maxillary bone, in consequence of its being extensively affected with the above disease. It was a most formidable operation, and the first time that it has been performed in England. The patient is still in the Hospital, is doing remarkably well, and the scientific practitioner might feel gratified in having an opportunity of seeing her.

THE LANCET.

London, Saturday, March 31, 1827.

The Number of the Yellow Journal for the present month (March) is equally as uninteresting as the most worthless of its predecessors. It is dull, stale, and unprofitable, and in going from Johnson's "Blue" to Roderick's "Yellow," it is a change merely from "chaff" to "bran." We are heartily sick of the insipidity of both, and implore them, if they have one spark of wit or a single grain of benevolent feeling, to supply us on future occasions with something of a more agreeable nature, than the recent results of their editorial cookery. Too much of *hotch-potch*, like too much of any other good thing, must in the end prove not only annoying but pernicious. It is the precursor of inveterate indigestion; hence the morbid inflation of the "blue" editor's entrails, and his lachrymose effusions on *gas*. The chief ingredients of the *hachée en pot*, now before us, are styled "Original Papers and Cases" on "Erysipelas," by Mr. Arnott, "Distortions of the Spine," by Mr. Shaw, "Treatment of Fractures," by Mr. Chandler, "Wounded and Diseased Arteries," by Mr. Travers, "Tumor in the Spermatic Chord treated as incarcerated Bubonocele," by Mr. Jeffreys, "Fatal Case of Chorea," by Dr. Hawkins, "Cases of Injuries of the Head," by Mr. Nobody, and, lastly, on the "Inefficiency of the Act of Vomiting, in removing Arsenic from the Stomach," by Mr. Scott. At the bottom of this list, which we have copied from the last page of the cover, the following significant notice is attached in a parenthesis, thus ("For various other original articles see our Intelligence") precisely what we would like to see; and, in fact, it is just what we have been looking for most anxiously, during the last three years, in vain, which we now despair of ever seeing, and therefore shall never search for it again. However, under the misapplied head "Intelligence," a few other slops are thrown in to complete the medley. "A Monthly Report of prevalent Diseases," "Case of Obliteration of the Aorta," by Mr. Churchill, "Remarks on Inflammation," by Mr. Wise, "On Salivation from inhaling Mercurial Vapour," by Mr. Somerville, "On the Utility of fumigations," by Mr. (not J. H.) Green, and "On Constitutional Effects from the external Application of Belladonna," by Mr. Wade.

In noticing these articles we shall not restrict ourselves to the foregoing arrangement, and on some of them we shall offer no comments at all, as they are beneath all criticism. Mr. Arnott's paper on "Erysipelas, accompanied by Affection of the Throat," occupies no less than eighteen pages, and in recapitulation the author says,

"On reviewing, therefore, these observations, and for the reasons developed in the course of them, I would venture in conclusion, to submit—

"1. That the term erysipelas should be restricted to that febrile affection of the system, accompanied with inflammation of the integuments of the face, to which it has most usually been applied; and that, until we have better evidence for so doing, the expression erysipelas and erysipelatous, should not be applied to affections of the skin in other parts of the body.

"2. That the term erysipelas phlegmonodes should be abandoned, as unnecessary, as well as inaccurate, and applied to dissimilar morbid conditions."

It surely is not necessary that we should insert any more of these "*propositions*." The first is absurd, and the second unintelligible. In the former, Mr. Arnott recommends that the term erysipelas should be "*restricted* to that febrile affection of the system, accompanied with inflammation of the integuments of the face;" and on the same principle, he of course would suggest the propriety of *restricting* "the term gout to that febrile affection of the system accompanied with inflammation of"—the *great toe!* This would be carrying us back a trifle,—only two or

three centuries or so to be sure. In a word, with the best feelings towards Mr. Arnott, we earnestly advise him to commence the study of Bichat's GENERAL ANATOMY, and in a very few weeks he will relinquish his scheme of *mapping* out the skin of the human body into *square inches*, and of applying to these little territories *dissimilar designations*, when assailed by the *same enemy*.

Mr. Chandler has invented a machine for the better security of fractured extremities, of the merits of which we cannot speak, as we have neither tried it nor seen it tried.

Dr. Hawkins's case of *chorea* proved fatal, notwithstanding "a full, but unsuccessful trial was made of the purgative plan of Dr. Hamilton. The patient had repeated doses of calomel, senna, or jalap; and an enema of oil of turpentine was three times administered." He does not state that purgatives were administered *daily*, the plan of Dr. Hamilton, but that she "had *repeated* doses," and we are justified in the opinion, that purgatives were not employed to a sufficient extent, from Dr. Hawkins's own assertion, that when given, "they NEVER failed to bring away COPIOUS and *dark-coloured* evacuations;" it would, we think, have been but consistent with "sound" medical knowledge, if the purgatives had been continued until NO MORE "*copious dark-coloured evacuations*" could be procured, and probably Dr. H. now entertains a similar opinion. Of the propriety of the practice of almost *daily* administering purgatives in chorea, agreeably to the proposal of Dr. Hamilton, we can speak with much confidence, as we have seen it attended with the greatest success.

In the paper of Mr. Nobody, on "Injuries of the Head," we are gravely told that those cases "which have been denominated *concussion* and compression of the brain, have many symptoms that are common to both;" and in that of Mr. Scott, that the act of vomiting is often insufficient to cause the ejection of *arsenic* from the stomach, which important facts we apprehend were pretty well known to the "sound" chirurgicals of *Kamschatka* some fifty years ago. Mr. Churchill's case of alleged obliteration of the aorta might have remained in Dart Street amongst the "botanicals," without any great injury to medical science; he himself allows that "the *pathological descriptions are obscure*, and difficult to comprehend." Why publish, then? The "remarks" of Mr. Wise "on some of the phenomena of inflammation," are not *exercise*; the *belladonna* case of Mr. Wade "does not enlarge the capacity of the pupil;" and we have passed through the fumigating papers of Messrs. Somerville, Green, Moonahee, and Shastree, without being suffocated—no trifling escape. John Shaw, with his thousand distortions, has really become a complete *bore*. He claims pre-eminence as a spinal doctor; and in order to attain his object, he probably deems it necessary to abuse and misrepresent all other labourers in the same vineyard. The reclining posture, introduced by the excellent Mr. Baynton, is declared to be injurious, when constant; though, the fact is, according to uniform experience, directly the reverse. It is to be employed daily for certain periods, and is then beneficial. The critical moment is, of course, revealed only to the supernatural genius of our admirable Crichton. Gymnastics, too, are essential to success; but Mr. Ward neither directs them like an anatomist, nor varies them, with the requisite skill. Stretching the spine is also an indispensable operation, when done with weights and pulleys, but with the fixed machinery of Dr. Harrison, it is often beneficial. Really, Mr. Shaw, to be serious on a grave question, did you ever witness or practise these several modes which you venture to condemn in no measured terms? We rather believe, for our own part, that you have not; and, moreover, that this un-

generous opposition is kindled more by the mammon of self, than the disinterested emotions of a scientific mind. We feel no inclination to assume the offensive, and push our videts into the enemy's lines, otherwise we could easily prove, that Mr. Shaw's hotch potch processes, gleaned from every quarter, are an ignis-fatuus, which glistens at a distance, but neither warms nor renovates.

May we ask who is A. Shaw, the delineator of J. Shaw's two engravings of Mary Anne Roach's distorted back. It is of importance to ascertain, at the starting post, that the figures were faithfully taken from the girl's person; that they are, moreover, true representations of the deformity before the treatment began, and after it was concluded. We do not require to be told, that a complaisant artist can easily increase the distortion in one drawing, and reduce it in the other. It is also to our purpose to remark, that unless portrait painters will condescend to flatter their employers, they never succeed with the public. Is it then too much to suppose, that A. Shaw may have performed a friendly office to J. Shaw, on an occasion so flattering to his interest and his vanity. The thing is certainly not impossible.

After all, we maintain that little reliance is to be placed upon drawings made directly from the figure, and least of all when executed by friends or dependants. Casts taken by artists of reputation and experience are of a very different order and denomination. These models are true representations of the parts intended to be shown. They cannot err, and remain durable monuments of the objects portrayed. We remember to have seen, at the time, a very strong display of the lateral curvature described by Dr. Harrison, and exhibited in the *Yellow Fungus*. The distortion was much greater than in Mr. Shaw's patient, and the success was certainly more complete. It will be seen, on reference to the plates, that not a shadow of deformity remained at the end of the process; whereas, with Mary Anne Roach, the disease is far, very far, from being subdued. Why, in "comparing the plans recommended in England, with those pursued in Germany and France," did Mr. Shaw omit to notice Dr. Harrison's brilliant and unique display of success? Does he expect, vain man, per fas et nefas, to raise himself into eminence? If it be his ungenerous aim to prosecute such schemes by misrepresentation and detraction, we assure him that a discerning and liberal public will defeat his object, and convince him, that an open and honourable course is more likely to gain him the distinction to which he aspires, than a covert and dirty path. Dr. Harrison's casts were taken from the figure by Mr. Mazzoni, the eminent modeller, before and after the treatment concluded. We therefore do not hesitate to place the fullest reliance upon his engravings, while we must be permitted to receive those of Mr. Shaw, cum grano salis.

The last paper on which we have to offer any comments is that of Mr. Jeffreys, entitled an "Account of a Case in which a Tumour in the Spermatic Chord was complicated with Symptoms so strongly resembling those of Incarcerated Bubonocele, as to lead to an Operation by which the true Nature of the Disease was ascertained," which we published verbatim at page 805 of our last Number. This is not the least curiosity of the records of "Hole and Corner" Surgery; had this paper appeared originally in THE LANCET as an "unauthorised" production, there are not six Hospital Surgeons of this town who would have failed to declare, that it was a false and infamous libel on the splendid talents of Mr. Bandager Jeffreys; that it contained not a word of truth; that it was published for the basest of purposes; that it was our object to misrepresent the supporters of the College, and to render them objects of contempt in the eyes of all the general prac-

titioners in the kingdom. Had we been the first to publish the report, these denunciations would have been repeated at the dinner tables of all the "Pures," would have been vociferated throughout the squares of all our Hospitals, and would have been bellowed into the ears of the students in four-fifths of our recognised theatres of anatomy. Many would have believed the "Hole and Corner" gentry; but happily the credulous are not quite so numerous now, as they were at the commencement of our labours; consequently our task has become comparatively easy. Some forty months ago, the Hospital Surgeons of this town were looked up to by the profession and the public generally as the great lords of the surgical creation, and with the "Pure" family, were really objects of worship. No one stopped to inquire what their qualifications were, that commanded so much respect; which neglect of duty is the more to be regretted, as it has not only proved a prolific source of emolument to the undeserving, but also the remote cause of much human suffering. From all that we can learn, it appears that Hospital Surgeons have been objects of respect simply from the fact of their being in office, whilst the means by which they obtained their elevation were entirely lost sight of. This, however, is a subject which we shall not now discuss, as the manner of conducting elections of the officers of our Hospitals will engage much of our future attention; premising only, that the readiest way to their improvement would be a radical reformation in the government of our College. To return from this digression to the paper before us, we do not hesitate to assert, that the first six lines contain a statement more calculated to render the surgeons of St. George's Hospital objects of far greater contempt, than all the exposures they have experienced at our hands; true it is, that on all occasions we have been most anxious to adhere to facts, and never to condemn, except on evidence of an indubitable character. As it must be a source of satisfaction to a judge and jury, after having condemned a criminal to death, to hear from his own lips a confession of the justice of the sentence, and the truth of the testimony on which he had been convicted; so it is with us most gratifying to receive the confession of those delinquents whom we have been under the necessity of placing at the bar of public opinion. Here is the confession, and if it do not stamp the character of St. George's surgery as the worst in the kingdom; if it do not establish most incontrovertibly the truth of our remarks on the treatment of poor Wheeler and Hammond, of Mary Burnet, and other victims of "Hole and Corner" surgery; henceforth let words be devoid of meaning, or confessions of guilt be received as the only testimonies of moral excellence. Mr. Bandager Jeffreys *loquitur*. "Cases have not UNFREQUENTLY occurred, where another disease has been *mistaken* for *strangulated* hernia, and an operation performed for the relief of the symptoms. The MOST COMMON of these has been, where an ENLARGED ABSORBENT GLAND has existed in the usual situation of rupture,—or where there has been an *encysted hydrocele* in the spermatic chord,—or where THE TESTICLE has been *stopped at the ring* of the external oblique muscle in its descent into the scrotum." This is, indeed, a pretty confession from a London Hospital Surgeon. ENLARGED ABSORBENT GLANDS are frequently mistaken for strangulated hernia, and an operation performed for the relief of the symptoms! (the *relief* of the symptoms too!) This is the state of operative surgery at St. George's Hospital A. D. MDCCCXXVII.

Here we ought to conclude this notice, but we must add another word or two on the peculiarities of the glandular hernia now before us, the particulars of the case are the following, which we take from the confession:—"P.H., said he was a watchman; on his return home, on Saturday morning,

December 23, he was suddenly seized with violent pain in the belly, followed by continued nausea, having had no stools for two days before; the pain was referred PRINCIPALLY to the NAVEL and the lower part of the abdomen. By direction of a medical man who saw him, he took some pills and aperient medicine, but without effect; on Sunday he was bled in the arm, and took more purgatives; Monday, bled and purged again, and no better; Tuesday, he observed a swelling in the left groin, and thought immediately that he was ruptured. On *Tuesday*, that is three entire days and nights after the first attack of pain at the navel, he discovered the swelling in the groin, and on the evening of that day he had "FOUR OR FIVE WATERY PURGING STOOLS;" on the following evening, Mr. Jefferys thought it right to cut into the tumour in the groin, discovered *three days* after the attack, under the impression that it consisted of strangulated intestine; such is the report of Mr. Jeffreys, such is the aspect of the case, although concocted under the "innocent nose" of the bandager himself; and we will ask what would have been its probable appearance if sketched by the pencil of an "unauthorised" artist. We advise all persons having glands in the neighbourhood of Poupart's ligament, to bear in mind, that when enlarged, they are "frequently mistaken" for strangulated hernia, by the "Hole and Corner" surgeons of St. George's Hospital.

THREE or four Numbers back, we alluded to the declining condition of the Medico-Chirurgical Society, and expressed our sorrow at its rapid decay. We were led to inquire into the causes of its dissolution, and stated our decided conviction, that the chief of them might be found in the opposition of the Society to the publication of its discussions. We are not singular in this opinion; in fact, several of the then Council, who objected when we first commenced the practice, have requested us to renew it, with which we certainly shall not comply unless we receive the unqualified consent of the present Council for so doing; as it is, in every sense of the word, a private institution, and we can have no legal right to publish its proceedings. At the same time, we cannot avoid expressing our astonishment that a body of gentlemen, who profess to meet for a scientific and highly useful object, can object to the immediate diffusion of that knowledge to their professional brethren in distant parts of the world, which they impart to the Members of the Society during the discussions. It is with peculiar satisfaction that we direct the attention of the reader to the sentiments of the President of the London Medical Society, at page 849 of our present Number, not so much on account of the high encomiums bestowed on this Journal, (although we are not insensible to the approbation of such an enlightened and honourable man as Dr. HASLAM,) but because we feel assured his opinions will exercise a powerful influence over the proceedings of medical societies in general, and elevate them to that rank of usefulness and respectability, to which, from their declared objects, they are so justly entitled.

As we have received many complaints of the irregularity with which the INDEXES of this Journal have been supplied by the newsmen, we have, on this occasion, taken extraordinary pains, and been at a considerable expense (a portion of which we are compelled to throw upon the purchaser, but not, we trust, without its equivalent,) to furnish a most copious Index with this the *concluding* Number of the Volume; so that Volume XI. is now complete. We shall endeavour to accomplish this object on all future occasions. Let it

not be supposed that we plead guilty to the above charge of irregularity; on the contrary, we believe that no Journal in London, during the last two years, has been more regularly or more promptly supplied with its Index, which has been invariably delivered with the first or second Number of each succeeding Volume; the fault must rest with the booksellers or newsmen; certainly not with ourselves.

Examen de la Doctrine Medicale generalement adoptée et des Systemes Modernes de Nosologie, &c. Par F. J. V. Broussais. 8vo. p. 475 Paris, 1816.

Histoire des Phlegmasies ou Inflammations Chroniques, &c. Par F. J. V. Broussais. Paris, 1826. (Troisieme Edition.)

Exposition des Principes de la Nouvelle Doctrine Medicale, &c. Par J. M. Goupil. 8vo. p. 622. Bailliere. Paris, 1824.

Pyretologie Physiologique ou Traité des Fievres Considerées dans l'Esprit de la Nouvelle Doctrine Medicale. Par F. G. Boisseau. 8vo. p. 722. Bailliere. Paris, 1826. (Trois. Edition.)

Medical History furnishes abundant examples of the perpetuation of error through prejudice and ignorance, and of the opposition which the most palpable truths have experienced, from men desirous of being regarded as medical philosophers. We may refer, by way of illustration, to the tenacity with which the Brunonian system was so long defended against all the evidence of common sense, and to the reluctance with which it has been forsaken. No system was more plausible at first sight, or better calculated to entrap the unwary: it procured proselytes by its simplicity, and its speciousness deceived those whose time, or want of inclination, did not allow of an immediate scrutiny into its absurdities. At the time of its appearance medical opinion lay prostrate before the chairs of Cullen, Hoffman, and Stahl, or, in other words, medicine was smothered with a scholastic jargon, which even its teachers acknowledged they did not understand. In this state of uncertainty, the ingenious hypothesis which reduced diseases into two great classes, and the treatment into an equally simple division, was caught at with avidity, and spread from country to country, until all Europe was desolated with a doctrine which destroyed former errors, by substituting others still more dangerous and absurd. For a time Brunonianism flourished; scarcely a medical school existed, in which it was not publicly taught; and scarcely could a practitioner of eminence be found, who, if he did not vindicate, insensibly fell into its incendiary practice. It was in the full stream of popularity, in France, when Broussais entered his protest against it, and warned his countrymen of the errors they were committing. This was first done about twenty years ago; and although it is difficult to make the voice of one man heard in a multitude, it must afford great satisfaction to that individual, to perceive that his remonstrances served to check the progress of that infatuation, whilst the gradual increase of the opinions he advocated, appears to show that the arguments on which they rested were satisfactory and convincing, that they were, in short, founded in truth.

Whilst this revolution of medical opinion was going on in France, a similar attempt was commenced about the same time in Germany, by Professor Marcus,* of Bavaria, and both were preceded by the efforts of an individual in this country, who, by a sober and inductive reasoning, arrived at

* Entwurf einer speziellen Therapie, 1805. Nürnberg.

the same general conclusions respecting the fallacy of previous opinions, and was, although unconsciously, co-operating to produce a salutary reform in the practice of physic.

Such was the state of medical opinion at the commencement of the present century; diseases were considered purely abstractedly. Did the symptoms indicate weakness, stimulants were poured in—did they exhibit proofs of increased force, depletion was employed, without any reference to the cause, or to the condition of the suffering tissue. We may best learn the tendency of medical practice from the treatment pursued in febrile affections, and perhaps from none so satisfactorily as from the management of that variety which, by common consent, has been called typhus, or typhoid, by which a state of weakness and oppression of the animal powers, no matter how induced, has been generally understood. According to the directions of the Brunonians this weakness was regarded as requiring the direct application of stimulants, and men dosed their patients with spirituous potations in typhus fevers, so called, and, in short, in all diseases where the much dreaded *asthenia* made its appearance. This empirical practice was tried, and innumerable were the victims which it sent down to Orcus; so that now it is rare indeed to find a man with hardihood enough to follow such a destructive career. The men who now give the tone to medical opinions in their respective countries have abandoned it.—Hufeland, in his Essay on Typhus, labours to prove to his countrymen, that the constitution of diseases has changed, and that typhus has become inflammatory.—Menderer, physician to the Russian armies, has asserted, that the stimulating practice was horribly destructive in the remittent typhoid fever of Moldavia, Wallachia, and Bessarabia, and he confesses that he was compelled to change it for the evacuant mode of treatment, and that he found this the safest, even when continued up to the period of convalescence. Hildenbrand,* who was at the head of the Vienna school, whose excellent treatise on Typhus is duly estimated by his countrymen, calls the state of reaction the inflammatory period, and Horn, of Berlin, asserts just the same thing.—Tomassini and Rasori, in Italy, have been forced by experience into the same confessions respecting the nature of the fevers called yellow and typhoid, reducing them to local inflammations, so that if we take a general view of the present state of medical opinion, what do we perceive? That thinking men of all countries have thus far approximated to throw down, as if by common consent, the bugbear of fever existing as a *distinct essence*, and this apparently without any communication or collusion, being individually induced to do so by the evidence of experience. They have been led to connect disordered symptoms with disordered function, and have been reasonable enough to be guided by the light of physiology and anatomy to the suffering structure. They have dispelled the incubus that oppressed the moral energies of Æsculapian reasoners, and have reduced a destructive nothing, an all-pervading universality,† into submission to the ordinary laws of matter and vitality.

This was the first step to the rescue of medicine from empiricism, and this is the only

* Uber den ansteckenden Typhus, nebst einigen Winken zur Beschränkung oder gänzliche Tilgung der Kriegspest und mehrere andere Menschenseuchen. Wien, 1814.

† "A fever" in the language of Dr. Fordyce, "is a disease that affects the whole system; it affects the head, the trunk of the body, and the extremities; it affects the body, and afterwards the mind; it is therefore *a disease of the whole system*, in every kind of sense."—1st Dissertation on Fever, p. 28. After this confession, he needed not certainly to have told us, that "it is a disease, the essence of which is not understood."—p. 118. According to Cullen, a collapse of the energy of the brain is the proximate cause of fever, which, extending its influence over the whole system, occasions "*a universal debility.*"

mode of investigation which can lead us to truth. We must proceed from general principles to special, and then trace their modifications and peculiarities. This is the manner in which Broussais has conducted his examination of medical opinions, and we scarcely remember to have read a work in which the efforts of a powerful mind were more singularly conspicuous. This Broussais, this vilified *gastro-enterite* Broussais, will be hereafter acknowledged to have been a man worthy of the appellation of a first rate medical logician, and to have befriended the human race by the analytical and sound reasoning which he has introduced into physic. At any rate we may say thus much, that he is an original, thinking, and clear-headed man; that what he has asserted is in conformity with experience; that he may therefore safely appeal to experience, and to all men who consult that sort of authority, for confirmation of the accuracy of that doctrine which, on the Continent, he was the first to teach and to oppose to a deluge of error. This doctrine we shall now shortly describe, and it will be seen from the mere sketch which our limits allow us to give, how sadly it has been misrepresented, and how closely it corresponds with the testimony of those whose testimony is worthy of respect.

The first part of the *Examen* exposes with the greatest ability the absurdity of calling fevers essentially typhus, in which debility or asthenia may sooner or later appear, and points out with great perspicuity that such a condition is to be regarded as a consequence, not as a cause. We hear, says Broussais, the words *adynamie* and *ataxie*, corresponding to nervous and muscular debility, in continual use, and because these conditions are found toward the close of most acute affections, they are regarded as two evil genii concealed behind the curtains of the patient. In order to prevent their appearance, evacuations are most rigidly abstained from, and the disease is allowed to go on, trusting to nature to bring about a *crisis* to accomplish the cure. Luckily for those whose complaints are of a slight character, such a termination does at last arrive, but much later than if the symptoms had been combated in a reasonable manner at the commencement; but when the disease is of a more important kind, by this expectant system, the powers of re-action gradually diminish, symptoms of prostration make their appearance, and then tonics and stimulants are poured in to accelerate the destruction.

The sarcastic style in which the practice of the old school is exposed, is so apt, and conveys such a true description of the false reasoning employed by its disciples, that we cannot do better than to introduce the author of the Examen, represented as combating the prejudices of an opponent. Having endeavoured to point out the extreme folly of substituting names for things, and of resorting to confused systems of nosology for an explication of symptoms, he says,

"We will suppose a case of incipient fever in which, from the first, there are symptoms of increased sensibility of the stomach; the lingual mucus is altered in appearance, and the taste is bitter; the case is then called a *disorder of the stomach*, and it is to be relieved by a good emetic; they take this as the only thing to be removed. Sometimes it so happens, that this disordered state of stomach does, in point of fact, depend upon irritation of foreign matters in the stomach, and the patient is relieved by the emetic, especially old people, the poor, the badly nourished, and those shut up in large institutions. But if the subject be young, irritable, or should have an irritation of the stomach in a little higher degree, there may be a little temporary relief gained by the emetic, but the mischief it has done soon becomes apparent, the fever develops itself on the following day, the tongue becomes dry and red, the thirst becomes intense, the general uneasiness and sympathetic pains of the muscles very considerable. Our philosophers then declare their having removed, by analysis of the symptoms, the *embarras gastrique*, and that they have discovered the concealed *fievre gastrique*. They do not see that by their treatment they have only exasperated the irritation of the stomach, and converted it into absolute inflammation of that organ. But the moment you say this, they reply that you can see nothing but a gastrite, that there was no sign of inflammation

whatever; that it was a fever; that in gastritis there is vomiting, and to prove it, they quote cases of poisoning by corrosive substances. It is in vain for you to reply, that the inflammation of a membrane does not cause the same pulse, or the same degree of heat, as the inflammation of a large bundle of capillary vessels in a more resisting texture. It is in vain for you to remind them that every body does not vomit; that inflammations have a thousand degrees; that it is not always equally painful; that it does not always arouse the same disordered sympathies. They treat all these physiological distinctions as subtile explanations. They say, *fever depends upon irritation, not on inflammation.* If you ask them to trace to you the line of demarcation between irritation capable of exciting fever, and inflammation, they reply that you wish to go into vain explanations, and to give vent to your imagination; and, still insisting upon the necessity of preserving the *strength* of the patient, they go on waiting for the natural crisis, doing little or nothing. In about seven or eight days, if the irritation is not allayed, the force of the pulse diminishes, the patient begins to feel very debilitated, and becomes taciturn; they turn round on you then in triumph, and exclaim, 'See, we have done well to husband the strength; see, here is *adynamie*; quick, quick, give tonics, camphor, quinine, serpentoria, and apply blisters.' This is the way in which they treat their patients, and they die. You open the body, you show them the mucous membrane of the alimentary canal, red, or even black from the cardia to the cœcum, and sometimes further; but it does not satisfy them; they say such things are usually observed in adynamiques, or low fevers."

Nothing, however, is more incorrect to conclude, as many have done, that Broussais limits the seat of fever to the mucous membrane of the intestinal canal; although he shows that in France such is most frequently the case, which may be easily accounted for when we take into consideration the predisposition of that structure occasioned by the habits of the people. He affirms that the brain is frequently the seat of fever, that any part of the body may in fact be the seat of the inflammation which gives rise to the fever, but the leading point for which he contends is, that there are no fevers without inflammation, and hence the absurdity of the stimulating plan of treatment. Whenever I perceive, says Broussais, a patient labouring under the symptoms of fever, without any particular pain that might connect it to the phlegmasia of authors, I do not rest content to regard it, as it is said to be, independent of inflammation; but I hunt out the seat, or rather the point of irritation which occasions the febrile movement. Show me, says Broussais, the organs; let us carefully examine their functions, and then if we find any one organ suffering more than another, let us endeavour to relieve it; we may find the brain and alimentary canal suffering together, and either consecutively; the stomach and bowels in consequence of the disease in the brain, or vice versa; and no matter by what name the fever has been called by nosologists, let us carefully and minutely search out the condition of the suffering organs, and not be led away from the physiological treatment of the case by the mere consequences of the disease. What can be more reasonable or philosophical; yet it is a common thing in this country to mention Broussais in connexion with some contemptuous epithet, and to repeat the eternal phrase with which men who will not take the trouble of investigating, affect to sneer at his opinions, "La gastro-enterite est la base de la pathologie." Whereas, nothing is more absurd.

To return to Broussais' notions of typhus fever. He does not regard, as we have before mentioned, as cases of typhus, such in which symptoms of debility may supervene during the progress of sporadic fever, but esteems that only as true typhus, which is produced by a miasm, the product of the decomposition of organic matter, or which emanates from a person already labouring under the consequences of an exposure to such an agent. To suppose otherwise, he contends, is not only chimerical, but worse, positively injurious, since it deters practitioners from combating the topical inflammation with energy in the commencement, and tends to accelerate the progress of the

disease by the premature administration of stimulants. He describes the impression made by this agency on the system, to be sometimes so powerful, as at once to destroy life without any reaction whatever; but that if the action of the poison (and he describes the miasm as a poison) should not be so intense as to prevent a reaction, or allow time for the judicious interference of art in the administration of stimulants, to assist in overcoming this congestion, that then the febrile reaction does take place, and that organ suffers which is most predisposed by any former weakness. It may be the brain, the lungs, or the abdominal viscera, or any other organs, but principally the three first, which must be treated on the same principle as any other fever produced by any ordinary agent, namely, as a local inflammation, with due care, however, allowance being made for the peculiarity of the agent now in operation. In typhus, therefore, when the febrile reaction is developed, the main danger is *not weakness*, but disorganisation of the principal organs, and more especially of the three before named. These are almost his own words, written, it must be remembered, in 1808. The following passage is so excellent, that we are tempted to give it verbally :—

"Ce qui distingue les phlegmasies typhodes de toutes les autres, c'est la facilité avec laquelle l'excitation organique cesse, et fait place à la torpeur dite adynamique. Or, plus l'irritation est considerable, plus ce funeste changement est prompt, comme on l'observe dans les inflammations indépendantes des miasmes délétères. Le premier soin du medicin doit donc être de préserver ces trois foyers principaux contre l'excès d'irritation qui menace de les detruire; s'il les preserve de la disorganisation, il previent necessairement la prostation générale qui marque *le second periode* du typhus."

Not that Broussais prohibits altogether the use of stimulants, or supposes that evacuants are to do all; he adopts no exclusive practice, he modifies it according to the exigencies of the case, and he points out the particular conditions in which a stimulating treatment is absolutely required, and mentions among others the following; In the general stupor which succeeds the first influence of marsh and other miasms upon weakly constitutions, where the powers of reaction are feeble, where there is a general sort of debility to be carefully distinguished from that state occasioned by any phlogistic action in any of the three principal cavities; when these measures, instead of rendering the tongue dry and coated, the thirst more ardent, the skin more hot, procure a diminution of these symptoms, and dispose to a general diaphoresis; when the febrile period terminates, and when the patient falls into a state of extreme feebleness, which cannot be attributed to the oppression of any particular organ, and which is, in point of fact, the first period of convalescence; Broussais then inculcates the necessity of carefully graduating the doses of the stimulants employed, lest the exaltation of the vascular action may be effected too suddenly, and the state of inflammation be again brought on. And he concludes his remarks on this subject, in the following animated and judicious passage :—

"As to the rest, it must depend on the sagacity of the physician; but it must be always borne in mind, that it is by applying our physiological knowledge to the sufferings of our patient, that is to say, by applying it without any exclusive system to the analysis of functions, to the study of sympathies, to the appreciation of the effects of modifying agents, that we only can arrive at that dexterity; and never by studying diseases as abstract essences, or by refusing to submit them to physiological analysis. But it is impossible to find anything but pure abstractions, speculations, and, in fact, absolute nothings, if we adopt the language of the Brunonians, or of those nosologists who affect to give a high degree of importance to artificial denominations systematically arranged, subdivided into pedantic classes, orders, genera, species, and varieties.

This is such a plain, reasonable, and matter of fact statement, that we need not to say more than to refer to the great similarity, we may almost say, identity of doctrine

taught on this side of the channel for so many years. Dr. Clutterbuck says, and has encountered much ridiculous opposition for saying so, that fever is always connected with inflammation, and that the seat of inflammation in *idiopathic* fever is the brain, but that in *complicated* fever, any other organ may be primitively affected, and the brain secondarily: which is no more than saying that in fever the brain is sometimes inflamed, and sometimes another organ; because the definition he has given to idiopathic fever, makes it an inflammation of the brain, and his complicated fever is an inflammation of any other structure. Broussais does not *call* inflammation of the *brain* idiopathic fever, but what amounts to the same thing, he says that the brain is sometimes primarily inflamed in fever; more frequently consecutively, but that there is no fever without inflammation. Therefore there is no real practical difference between these distinguished practitioners; it lies in the mere use which Dr. Clutterbuck makes of the word *idiopathic*. The merit of originality appears fairly to belong to each of these gentlemen, as it would appear from reference to dates. Dr. Clutterbuck first broached his opinion on fever in his inaugural dissertation, "De sede et Natura Febris;" published in the year 1804, and the first edition of his "Inquiry into the Seat and Nature of Fever," in 1807. Broussais' "Histoire des Phlegmasies," was first published in 1808, and it does not appear that either was acquainted with the labours of the other. Be that as it may, their efforts have tended to remove the vague notions respecting fever before in vogue, and have led to a decided and rational practice. With the following extract we must conclude this part of the subject.

"Having compared the results of my practice," says Broussais, "with that of others, I may assert, without fear of contradiction, 1st, That typhus is not in reality so frequent as it is represented; 2d, That the most part of the pretended gastric fevers are only slight inflammations, which are exasperated by the use of emetics, and tonics, and acrid purgatives; inflammations which could most frequently be arrested in their march by the antiphlogistic treatment, and which do only present the symptoms called *adynamiques* and *ataxiques*, because nature has been allowed to labour unrelieved under the irritation, or because the febrile state has been maintained by irritants; 3d, That after reaction has taken place in true typhus, in contagious typhus, the best mode of stopping its progress, or of rendering the symptoms less severe, is to treat it by evacuents, and acids, to proscribe all aliments, and to have recourse to local or general bloodletting, if the irritation in the viscera should require. Cerebral and pectoral irritations are common in typhus. The first, ordinarily consecutive to that of the alimentary canal, is, however, very frequently (fort souvent) primitive in the spring and during summer; the second presents itself almost always in winter, and the extreme mortality of the typhoid epidemics in cold countries is, in great measure, owing to it."

Thus we see, that respecting the treatment of fevers in general, and of typhus in particular, there is little or no practical difference between Drs. Clutterbuck and Broussais. With regard to intermittent fevers, whether benign or malignant, Broussais regards their pathology as that of inflammation, and considers that the access may be explained in the following manner:—Every *intermittent fever* presents marks of a temporary *congestion*, and that congestion will be greater or less in proportion to the degree of strength of the patient; that it afterwards passes over into a state of excitation, which may act more upon one organ than another; that, by a repetition of attacks, that excitation may pass over into *inflammation*; that they may and frequently do pass into *continued fever*; and that there is, in short, a most intimate connexion between the two *. That as the

* Dr. Gideon Harvey, in his very curious tract, "A Discourse of the Plague," which was published soon after the great endemic of 1665, states distinctly that "The disease did oft appeare at first as a *diary feaver*, which speedily did go into a putrid feaver, suddenly accressing into a most malign and pestilent feaver." He

inflammation in these cases is usually of a less vigorous kind than under other circumstances, the constitution appears to remove it periodically by a certain crisis, but that this it is not able to accomplish perfectly, and that then *bleeding*, to a moderate extent, will often succeed in removing it altogether. Broussais contends, that stimulants should be used with caution, and never be employed excepting in the intervals of the accessions; but that the most safe and discreet treatment is to calm the morbid irritation and to direct the attention to the viscera; seeking rather to remove that condition than to trust to the usual empirical mode of procedure, which, as he says, is often "*un jeu du hasard, une partie d'honheur à quitte ou double.*"

This, as far as our space would allow, is a sketch of the predominant opinions of the great Reformer of French Medicine — of the Founder of the "*Medicine Physiologique.*" The work of Goupil contains a valuable body of facts, collected in the Theses of the French Students, bearing upon the leading points of Broussais' doctrine; that of Boisseau is a much more complete work, and furnishes a very good exposition of the opinions of the new French school. It would be well received in an English dress.

Remarks on the Merits and present State of Vaccination, wherein the Objections of the Antivaccinists are considered. By ROBERT LEWINS, M. D., &c. 8vo. pp. 80. Edinburgh, 1825. Constable, and Co.

Dr. LEWINS has put together most of the arguments which have been adduced on the side of the vaccinists and the antivaccinists; 1st, taking a cursory view of the history and effects of the small-pox; 2dly, giving a succinct history of vaccination; 3dly, considering the objections of the antivaccinists; 4thly, inquiring why vaccination occasionally fails; 5thly, remarking on the present state and protecting power of vaccination; 6thly, examining the proposal of substituting variolous for vaccine inoculation; and lastly, appending the report of the Vaccine Institution of Edinburgh, for 1824, as also that of the Hospital at Battle Bridge, for the same year, which appeared in the seventh volume, p. 21, of THE LANCET. The author is a practitioner of repute, at Haddington, North Britain, where it seems certain prejudices still exist on the subject of vaccination; and his work is not so much designed for the profession, as in the language of his preface, "To confirm the faith of the wavering, or to restore to the cause which he advocates, that confidence which would never have been shaken, had men taken reason for their guide."

So much has been said and written on vaccination, that it were to little purpose to go into detail on the subject. The little work of Dr. Lewins is calculated to effect the benevolent object which he had in view; and by no critical rules has the unassuming author merited the censures of the younger Duncan's reviewer, of whose conduct, therefore, we think he has righteous grounds of complaint.

A Portrait of GEORGE BIRKBECK, M.D. F.G.S. M.A.S., President of the London Mechanics' Institution, and of the Medical and Chirurgical Society of London, &c. Knight and Lacey, 1827.

Dr. BIRKBECK belongs to a class of worthies who seem born for others rather than themselves. Although time, the destroyer of all

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also appears to have distinguished justly the different forms of attacks, for he says, "That in some the feaver was high on the beginning, whence," says he, "nothing is more proficous against the plague (but in the commencement onely) than phlebotomy." Distinction xviii. p. 153.

things, should lop the Mechanics' Institute in the dust; the impulse which has been communicated to the sciences will be felt by distant ages, and perpetuate the genius, and the philanthropy of its founder. Not satisfied with contributing munificently to the funds of the Institute, he has laboured incessantly in the lecture-room; taught the laws of mechanical philosophy to thousands, who but for him could have never hoped to attain them—more than all, by his example, which has been followed in most large towns in the kingdom, he has elicited a spirit of emulation and inquiry, which cannot fail to be of lasting service to science, and the nation at large. The plan which we lately submitted to our readers, of giving popular demonstrations on anatomy, is about to be reduced to practice by this real friend of the human race, and will assuredly dissipate the prejudices which at present exist against the most useful, interesting, and humane of the sciences, without which there is no certainty in physic. Dr. Birkbeck has deserved well of his country, and it therefore gives us great pleasure to state, that his likeness is very cleverly delineated in the engraving before us, which, though his name will never die from the lips of mechanics, will give a forcible idea of the man, ornamenting alike the room of the artizan, and the cabinets of the philosopher, and friend of science. No one who has seen the Doctor, could fail to recognise the fidelity of the copy. The face is richly intellectual; and, indeed, the whole contour of the picture is highly creditable to the artist's talent. It is engraved in mezzotinto by Dawe, from a painting by Lane, and its size is fourteen by seventeen and a half inches.

Ben Travers, at the St. Thomas's dinner, in a raving speech, told the company that "He was determined to do his duty through good report, and through *evil* report;" would he had added, "through the PERINÆUM!" We find he could have spared his anxiety on the subject of *nitrate*, as the landlord, knowing his company, had taken the precaution to *store* all the raisins on the table.

On going round the wards of the Middlesex Hospital a few days since, Mr. Joe Burns was informed by one of his patients, an elderly woman, that she had "*a swimming in the head*;" "Let the physician see that poor woman," said Joe, turning round and addressing the nurse, "Let the physician see her; depend on it, she has got water in the head, or else she would not have a *swimming* there!"

ZOOLOGICAL SOCIETY.

A Meeting of the Members of this Society, which was instituted some months ago for the advancement of zoology in this country, was recently held in Bruton-street, for the purpose of appointing a President in the place of the late lamented Sir T. Stamford Raffles; when the Marquis of Lansdowne was unanimously elected President of the Society. We learn from the Report which was laid on the table, that the plans of the Society are in considerable forwardness, and are likely to be carried into complete effect. The Museum in Bruton-street (consisting of extensive collections in every branch of zoology) is open to the Members; and preparations for a garden and menagerie, on the ground allotted to the Society in Regent's Park, are going on with activity. It is expected that the gardens, with promenades, plantations, aviaries, and sheds, for the more interesting quadrupeds and birds, ponds for fish and wild fowl, &c., will be opened in the course of the ensuing summer. Every exertion is making to render this establishment one of the most splendid and interesting attractions of the metropolis.

THORACIC VACUUM THEORY.

Suum cuique.

To the Editor of THE LANCET.

SIR,—As you were pleased to deem the discussion which took place on the Thoracic Vacuum Theory at the London Medical Society of sufficient interest to publish; and as Dr. Barry at that discussion endeavoured to maintain that "no other man, either living or dead, had *even imagined* experiments similar to his," to show the nature of the *mechanism* employed to form that vacuum; I consider it but justice to that distinguished and indefatigable physiologist, M. MAGENDIE, to send you a copy of a letter which I have recently received from him on this subject: especially since Dr. Barry undertook to say, that "M. Magendie had *no very accurate notions* of the effect of the expansion of the chest on the *sanguiferous* cavities. It will be seen by referring to the 179th No. of THE LANCET, that I endeavoured to show by some quotations, which I read from the second edition of M. Magendie's Elements of Physiology, that he had not only a VERY *accurate notion* of the nature of the thoracic vacuum, and of its influence on the return of the venous blood, but that he had arrived at that accurate knowledge *through experiments* made on the jugular and crural veins of animals, by inserting tubes into them, the other extremities of which were placed in connexion with coloured fluids; in short, in precisely the same way as was adopted by Dr. Barry. At the time the meeting took place I had not the first volume of M. Magendie's "*Journal de Physiologie*," published in 1821, at hand, but I have since procured it, and have satisfied myself of the identity of the description there given, of the influence of the thoracic vacuum on the circulation, with that found in the second edition of his Physiology, published in March 1825, from which I made the quotations; indeed, the one is a verbal transcript of the other. Dr. Barry's experiments were made in 1824, and the little brochure in which they were described, was published in the autumn of 1825. As M. Magendie has been so polite as to answer the questions I addressed to him, seriatim, it is unnecessary for me to repeat them. The following is a copy of his letter:—

MONSIEUR,—Vous me faites l'honneur de me demander:—

1mo. A quelle époque et où j'ai demontré par l'experience l'influence de la respiration sur le cours du sang veineux.

Dans 1817 j'avais communiqué à l'Academie des Sciences un premier mémoire sur la circulation, dans lequel je traité de l'action des arteres. (Journal de Physiologie, 1re Vol. p. 102.) Dans un second travail j'ai traité de l'influence de la respiration sur la circulation. Ce memoir a pour titre, "*De l'Influence des Mouvemens de la poitrine sur la circulation du Sang*;" (Journal de Physiologie 1821, 1re Vol. p. 131.) Les experiences qui se trouvent a la page 136, se rapportent entierement a la question traitée par Mr. Barry. Enfin, dans le meme volume de mon Journal, page 190, se trouve un "*Memoire sur l'entrée accidentelle de l'Air dans les Veines, sur la mort subite qui en est l'effet, sur les moyens de prevenir cet accident et d'y remedier.*" Je y demontre que l'entrée de l'air est bien evidemment un effet d'aspiration, et par conséquent de pression atmosphérique.

2do. Vous me demandez, Monsieur, si je me rappele le nom des Médecins ou Etudians anglais qui ont assisté à ces expériences; je les répète tous les ans dans mon cours de Physiologie experimentale, un grand nombre de personnes ont pu en être témoins.

3tio. En sortant de l'Institut apres la Seance dans laquelle Mr. Barry lût son memoire, je lui demandai seulement s'il avait connaissance des expériences de Haller, de Lamure, de Lorry, et des miennes. J'ai cité les travaux de ces auteurs dans mon "Mémoire sur l'influence des mouvements de la poitrine." Mr. Barry me répondit *qu'il ne les connaissait pas.* Je lui fit observer alors qu'il eut été convenable de prendre connaissance de ces expériences avant d'en entreprendre de semblables sur le meme sujet.

Je dit en outre publiquement dans la discussion qui eut lieu apres la lecture du rapport qui fut fait au sujet du memoire de Mr. Barry, qu'il etait *mécaniquement absurde* de regarder la pression atmospherique comme la cause principale de la circulation, et de ne pas admettre que le cœur y eut la plus grande part.

Je désire, Monsieur, que ces renseignemens puissent vous être utiles, et vous prie d'agreer mes civilités.

MAGENDIE.

Paris, 13 Mars, 1827.

(TRANSLATION.)

Sir,—You do me the honour to ask me:

1st. When and where I showed by experiment the influence of the respiration on the course of the venous blood? In 1817, I communicated to the Academy of Sciences a first memoir on the circulation, in which I treated of the action of the arteries. (Journal de Physiologie, 1st Vol. p. 102.) In a second paper I treated of the influence of respiration on the circulation. This memoir was intitled, "*Of the influence of the movements of the Chest on the Circulation of*

the Blood." (Journal de Physiologie, 1821, 1st Vol. page 132.) The experiments which are found at page 136, relate entirely to the question treated of by Mr. (Dr.) Barry. Lastly, in the same volume of my Journal, page 190, there is a "*Memoir on the accidental entry of the Air into the Veins, on the sudden death which is the consequence of it, on the means of preventing that accident, and of remedying it;*" I there show, that the entry of the air is evidently an effect of inspiration, and consequently of atmospheric pressure.

2dly. You ask me, Sir, if I recollect the names of the English physicians or students who were present at these experiments? I repeat them every year in my course of experimental physiology; a great number of persons have been witnesses of them.

3dly. In going out of the Institute after the sitting, in which Dr. Barry read his paper, I asked him only if he were acquainted with the experiments of Haller, of Lamure, of Lorry, and mine. I have quoted the works of these authors in my memoir, "On the Influence of the Motions of the Chest." Mr. Barry replied, *that he knew nothing of them.* I then observed to him that it would have been but becoming to have taken some notice of these experiments, before he undertook similar ones the same subject.

I said, moreover, publicly, in the discussion which took place after the reading of the report which was made on the subject of the memoir of Mr. Barry, that it was *mechanically absurd* to consider atmospheric pressure as the *principal* cause* of the circulation, and not to admit that the heart played the greatest part in it.

I hope, Sir, that this information may be useful to you, and beg you to receive my respects. MAGENDIE.

Paris, March 13, 1827.

This document, I think, Sir, confirms the assertions I made respecting the merits of M. MAGENDIE; and serves to show that *even the experiments* of Dr. Barry are *not original*; that the *mechanism* of the Thoracic Vacuum Theory was *long since understood*, and its influence on the circulation *sufficiently appreciated.* The thoracic vacuum can only be regarded as an inferior accessory power in the circulation: to suppose otherwise is *physiologically*, as well as "mechanically absurd." I remain, Sir,

Yours very respectfully,
EDWARD JOHN SPRY.

5, New North-street,
March 26, 1827.

*This was Dr. Barry's opinion. He has since given it up, as we find nothing said of atmospheric pressure as the "*cause principale*" in the English edition.

FOREIGN DEPARTMENT.

SURGERY.

Extirpation of a Sarcomatous Parotid Gland.

A SHORT time since we gave an account of the successful extirpation of this gland by M. Lisfranc, at La Pitie, in the present case Professor Nægele of Heidelberg was the operator. No further observations require to be now made on the practicability of the operation, since this case constitutes the fifth in which the gland has been removed with success.

Barbara Schackert, of Gaiberg, aged 45, the mother of many children, perceived about fourteen years ago a tumour to form between the mastoid apophysis and the angle of the jaw, which, although painful, she neglected. Finding that the tumour increased in size, she consulted a country surgeon, who applied a caustic to induce suppuration. It appears that the wound after a time was healed, and that there remained a little hard swelling in the centre of the original tumour. In the following year the tumour again increased, and became as large as before, when it was again treated with caustic. From the ulcer thus formed, a friable matter was observed to be discharged from time to time; the wound kept open, and could not be healed. Much dismayed at the rapid progress of the tumour and ulcer, the patient now applied to the surgical clinic attached to the University of Heidelberg. The swelling extended from the lobe of the ear to the middle of the cheek, and descended about four-fifths of an inch below the edge of the angle of the jaw, from which place it passed upwards and backwards, over the mastoid process. It was moveable to a certain extent, and by powerful pressure on its sides, it could be raised a little from its bed. In its centre was an ulcer with hard and inverted edges, almost as hard as the long circumference of an egg. The suppuration was abundant, and very fetid. The operation was performed on the 21st of May. The whole of the diseased skin was included between two semi-lunar incisions, and the integuments were dissected back to the base of the tumour. The operation occupied about a quarter of an hour; the bleeding vessels were secured as they proceeded, and the trunk of the carotid artery was exposed at the bottom of the wound. The hæmorrhage was not great, the edges of the wound were brought together as near as possible, and

secured by appropriate bandages. The dissection of the tumour showed in its interior a rosaceous mass, in which were contained several indurated points connected together by a dense cellular tissue.

The case proceeded in the usual way without any untoward circumstances, and the patient left the clinic on the 2d of June. She attended as an out-patient for some weeks, when the wound healed perfectly, leaving a small cicatrix.*

New mode of treating Wounds of the Intestines, as proposed by M. LEMBERT.

The ligatures used by M. Lembert may either be of silk or flax, and a number of needles being armed with these substances are to be passed through the intestine between its muscular and mucous coats, about two lines from the wounded edges. As many ligatures as may be deemed necessary are to be passed before any are tied, the index finger of the left hand having been previously introduced into the intestine, and the lips of the wound placed in juxta position. The ligatures are not to be passed through the coats, but between the muscular and nervous coats, and the point of the needle is to be again brought out about at a line from the edge of the wound. Introducing the needle on the opposite side at about one line from the edge of the wound, it is to be glided between the mucous and muscular coats, and brought out at about the distance of another line, so that about two lines of the cylinder of the intestine are not included in the ligatures, and when the ligatures are drawn together, the *surfaces of the serous membrane*, the peritoneum, *are brought into contact.*

The free edges of the wound are in this way turned toward the cavity of the intestine, where they project to a greater or less extent; the ligatures are to be then carefully tied, during which process, great care should be taken to preserve the serous covering of the intestine in contact. In the cases of transverse wounds of the intestine, the ligatures are passed in the same manner as in the longitudinal wounds just mentioned, taking the precaution to pass a ligature on each side of the mesentery, so as to make the two portions of intestine correspond exactly. The ligatures are then to be tied, taking care to bring the peritoneal covering in contact, and to turn the edges of the wound inwards.

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* Heidelberg Klinische Annalen, 1826. Von Prof. Nægele.

ROYAL INFIRMARY.

"Surgical Miscellanies, operative and controversial."

THE occurrences in this establishment, since the last notice of this description, have been more various than important. The omission, however, of a single scene might be detrimental to the unity of the farce—so you must take the performances, such as they are, as husbands take their wives, for better for worse. During the "great snow" Erysipelas made an irruption from its lurking-places to the great derangement of sundry ulcers, and the manifest danger of their possessors. The origin and cessation of this endemic, for such it may with strict propriety be called, in the Royal Infirmary, gave rise to some speculations. By some its disappearance has been attributed to the thaw, as its production was to the frost; by others, who supposed it to have been generated by an infectious locality, its intermission is set down to the credit of fumigation. A show of hands would, probably, decide in favour of the latter opinions. Without entering further on such abstruse questions, an architect or a new hospital would supply the best preventive to this periodic malady, which renders the performance of operations a hazardous undertaking within the walls of this institution. During the prevalence of the erysipelas, venesection was followed by severe inflammation. In one of these cases, the symptoms assumed a degree of even unusual violence. The subject of them had been in the medical wards for a chest affection, (pleurodynia), for which he was bled. Gangrenous erysipelas attacked the wound. For this he was again bled in the other arm, and here also the disease commenced, but in a milder form. The sloughing in the limb first affected, extended to about two inches in every direction from the lowest puncture, and ultimately denuded the vessels in the hollow of the fore-arm. The constitutional fever placed the patient's life in imminent danger for several days. Opium, quinine, and cordials, with fermenting poultices, were the remedies principally employed. The disease in this instance did not seem to have extended along the course of the vein, or of the lymphatics, being confined to the cellular substance. A more minute detail of the case would afford nothing with which the public are not already acquainted. The patient is at present convalescent, and the sore in a favourable state of cicatrization. Besides these cases, there were some amputations; the fore-finger was the object of one of them. On

removing it, the learned operator held up the part to the class, and with the utmost simplicity imaginable informed them that the bone "was in an incipient state of disease;" upon which some of the wags present declared, with some semblance of truth, that "prevention was better than cure." The second of these amputations was performed on a female *philanthropist*, who received a compound fracture in the exercise of her perilous avocations. The arm was removed above the elbow, shortly after the infliction of the injury. Scrofulous disease of the instep rendered the removal of the leg below the knee necessary in the third of these cases. The treatment of this stump bears so directly on the operator's defence of sutures in his celebrated clinical lecture, (which has been distributed for criticism or insertion among the Editors of the Medical Press of Edinburgh,) that you will excuse a recurrence to this topic. Dr. Ballingal, you are aware, appeals to the cases recorded in his lectures as proofs of the utility of sutures in amputation. The writer did not think that he would so soon be furnished with a practical answer to these appeals. The patient was a young man, in as good health as the presence of a chronic disease would permit him to enjoy; in short, he was, it must be allowed, a fair subject for the decision of an experiment. He was accordingly brought to the theatre; the single flap operation was performed; an unnecessary quantity of muscle retained; and the integuments consequently closed with difficulty, even with the assistance of numerous and deeply inserted stitches. All the pupils of the Royal Infirmary are witnesses of the consequences which have ensued. Inflammation came on in the usual time, about the third day; the stitches were torn asunder by the distention of the parts; the wound, after an interval of several days, continues up to the hour of writing covered by a dense investment of adhesive puriform matter; and the skin along the remaining portion of the tibia of an erysipelatous redness! Comments would but obscure so plain a text. The most interesting mistake of the week, however, occurred in the case of a diseased scrotum. It was considerably enlarged; its sides thickened, the affection extending equally to both cavities, and contained an abscess in the cellular substance. On emptying this by a trocar, the nature of the contents excited suspicions that the testicle was not sound. Its extirpation was accordingly determined; the incision along the course of the cord was made; but just as the knife denuded the testicle, it turned out as healthy and sound a testicle as any in the Royal Infirmary. The operator threw a look of condolence into the patient's face, and assured him that the part was safe for this time! A practitioner with iron fingers, would have perceived that the disease, in this instance, had not reached the testicle, without the assistance which the history of the case, its general appearance, and the information deducible from the puncture, afforded.

Edinburgh, March 24, 1827. SCOTUS.

LONDON MEDICAL SOCIETY.

Dr. HASLAM, President, in the Chair.

THE meeting of the Society, on Monday last, was exceedingly large; the members, probably expecting to hear a continuation of the discussion on the osseous union of fractures of the neck of the thigh bone within the capsule, and to see the specimens of Mr. Langstaff, produced.

The minutes of the former meeting having been read, Mr. Kingdon said, that he was desired by Mr. Langstaff, to inform the President and the Fellows of the Society, that he was accidently prevented from attending that evening; that he had selected some preparations from his museum, for the purpose of sending to the Society, and that he should certainly be present next Monday evening.

Mr. KINGDON then observed, that he was desirous of making a few remarks on the minutes which had been just read, more especially since some expressions had been attributed to him in a publication of the last week, which he was not conscious of having made.

The PRESIDENT, addressing Mr. Kingdon: Will you name the publication?

Mr. KINGDON. "THE LANCET." Not, said Mr. Kingdon, that I have any objection to the publication of what I may say in this Society; but I must protest against any imperfect or abridged statement of mine, upon this or any other subject, being made without my consent. I say, that I have no objection to the publication of my opinions, provided *all* that I say be stated and *no more*, and I should recommend, that if any gentleman present should feel inclined to report the proceedings of this Society for THE LANCET, that he be furnished with suitable accommodation for the purpose. This, Sir, is my opinion. Reverting to the specimen of the fracture of the neck of the thigh bone which was produced on a former evening, Mr. Kingdon wished it to be understood that he was perfectly satisfied of the fact of fracture having taken place within the capsular ligament, and of that frac-

ture having united by bone. He did not say, as it had been represented, that the close coverings of the bone were not torn through, but that at the time the fracture occurred, the pelvic portion was crushed or jammed into the femoral portion of the neck of the bone, and had been there retained until ossific union had taken place.

The PRESIDENT observed, that he was sorry to find that Mr. Langstaff had been prevented from attending, but hoped to have the pleasure of seeing him at the next meeting. He considered that Mr. Kingdon was perfectly right in making the observations which had just fallen from him, respecting the inaccuracies of the minutes, or of any published report; and this brought him to make a few remarks on the latter subject. He saw no objection to the publication of the proceedings of this Society, nor of any other to which he belonged; and as "THE LANCET" was now become the medium of communicating medical opinions, he, for one, thought that the discussions of the Society could not be transmitted in a better channel. He thought it right to say thus much, because it had been attempted to throw some obloquy on "THE LANCET;" and he only stated his firm conviction, when he said that it was not only the most extensively circulated, but the best conducted of all the medical journals, not only of this country but probably of Europe. He should be happy therefore to furnish every accommodation to any gentleman who might feel inclined to send reports of their proceedings to THE LANCET, or to give any person who might be sent for the purpose, a seat at his side; being well assured that no Fellow of that Society would state any thing there that he would be reluctant to see published. One of the main objects of the Society, was the *diffusion of medical knowledge*, and he did not see how this good object was to be better answered than by the *publication* of its proceedings; and he might say, without vanity, that no discussions were of a more interesting and useful description, than those which were held at the London Medical Society. (A strong feeling of approbation was manifested throughout the Society at the conclusion of this speech.)

Mr. LLOYD was at a loss to know how the fractured surfaces in the specimen of fracture of the neck of the thigh bone, shown by Mr. Amesbury, could have been jammed together, as was stated by Mr. Kingdon. If they had been so jammed, no shortening of consequence could have taken place, whereas it appeared certain, from the history of the case, that a very considerable shortening of the limb was observed immediately after the accident, and, in his opinion, the present appearance of the specimen did not warrant the conclusion to which Mr. Kingdon had arrived.

Mr. AMESBURY said, whatever might have been Mr. Kingdon's intention, he (Mr. A.) certainly understood him to say, that the neck of the bone might have been crushed in the specimen now under consideration, without a division of the close coverings of the bone, and, in consequence of this impression, he was induced to make the observations he did in answer to Mr. Kingdon. The Secretary also conceived, that this was the idea which Mr. Kingdon meant to state, as he had expressed it in the minutes.

Mr. KINGDON satisfactorily explained.

In consequence of a question from one of the Fellows, Mr. SHARP observed, that he had conversed with Mr. Chorley upon this case. Mr. Chorley had informed him, that the Gentleman lived eighteen months after the accident, and was able to walk so well for twelve months before his death, that many of Mr. Chorley's medical friends had some doubts upon their minds, whether the bone had ever been broken, but he could not say to what extent the limb appeared shortened after union had taken place. The gentleman had an awkward walk before the accident which seemed to arise from the infirmities of age, and he also limped a little after the union of the bone had taken place.

Mr. PETTIGREW was of opinion, that this was a fracture of the neck within the capsule, which had extended through the bone, and that ossific union had taken place.

Mr. GOSSET made some observations upon the position which the head and neck of the bone sometimes assume in old persons, and the difficulty of deciding in many cases whether the neck of the bone had been fractured or not; but we understood him to say, that this was a decided case of fracture within the capsule.

Mr. AMESBURY made some remarks upon fractures of the neck of the thigh bone generally. He said, that there were two parties who had opposite opinions respecting these cases, the one, of which Sir Astley Cooper was at the head, believed that union does not take place, and the other, that union might be effected. He confessed that he was disposed to favour the latter opinion. Sir A. Cooper has expressed his opinion very justly. He states, that he has never met with any case of bony union, and when we bring before our minds the great number of specimens which Sir Astley Cooper has collected, in which ligamentous union only is observed, Mr. Amesbury thought it could not be wondered at, if Sir Astley Cooper entertained the belief, that bony union cannot take place. Sir Astley Cooper had done much for the profession, by what he has said upon dislocations, hernia, and some other subjects not unknown to the Society; and although it was well known that Sir Astley Cooper had paid great attention to the sub-

ject of fractures of the neck of the thigh bone, when his extensive engagements in other ways were considered, he was by no means to blame if he has not weighed every part of this question in the manner it deserves. Sir Astley Cooper had not met with any cases of bony union, and he conceived that bony union does not take place, 1st, because the fractured surfaces are not kept in apposition; 2d, because they are not pressed together; and, 3d, because there is a deficiency of nourishment in the head of the bone. Sir Astley Cooper lays strong emphasis upon the last reason which he mentions, viz. the deficiency of nourishment to the head of the bone. This he considers to be the chief cause why fractures within the capsule do not unite, by the interposition of bone. With due submission, Mr. Amesbury wished to differ from Sir Astley Cooper in this particular; he believed that the deficiency of nourishment was the *minor*, not the *major cause*, of non-union in these cases. He was of opinion, that the want of union must be attributed more to the want of apposition and rest, than to the deficiency of nourishment in the pelvic portion of the bone. Sir Astley Cooper had, it would seem, overlooked the inadequacy of the treatment; which he (Mr. Amesbury) thought was very bad indeed; and he was prepared to show, and would show, at some future meeting of the Society, that the means commonly employed were very ill calculated to answer the indications which ought to be answered in the management of these cases. Mr. Amesbury observed, that splinters of bone entirely denuded of the periosteum, and lying loose, would accept of union in the same manner as a transplanted tooth, provided they were not deprived of their vitality; and knowing this to be true, he thought that the head of the bone was, under circumstances, much more likely to assist in the process of union, than a denuded and detached splinter. The pelvic portion of the bone, said Mr. A., is not only supplied with a sufficient quantity of blood for its support, but actually with enough to produce an ossific action upon the fractured surface, as is shown by the increased density which is observed in this part subsequent to the accident. This state of the fractured surface of the pelvis could only arise from a new deposition of ossific matter. He did not conceive that the synovia, getting between the fractured surfaces, would have much effect in preventing union in these cases, as it did not prevent union from taking place in the fractures of other joints, in many of which, the fractured surfaces must be covered with synovia shortly after the accident. The cause of non-union then in fractures of the neck of the thigh bone within the capsule was, in his opinion, not physical, but mechanical.

Mr. KINGDON concurred with Mr. Amesbury.

Mr. CALLAWAY said, that if want of apposition and rest were the principal cause of non-union, how did it happen that in fractures partly within, and partly external to the capsule, we found only that part of the fractured surfaces which was external to the capsule, united. There is a specimen in Guy's Hospital, in which this kind of union might be seen. Mr. Callaway contended for the honourable manner in which Sir Astley Cooper had conducted the inquiry respecting fractures of the neck of the thigh bone. He doubted the expediency of attempting to bring about bony union in those fractures which are within the capsule, and concluded by saying, that he saw no sufficient reason to induce him to alter the opinion which he had previously formed upon this subject.

Mr. AMESBURY hoped that the Gentlemen present had not attempted to cast any slur upon the character of Sir A. Cooper. (No, no, no, from several of the Fellows.) In answer to the observations which fell from Mr. Callaway, Mr. Amesbury begged to observe, that it could be readily explained from the state of the parts in the two situations, why, where there was a double fracture, one within and the other external to the capsule, that external to the capsule only was found united. Here Mr. Amesbury explained the difference to which he alluded, and said, that fractures partly within, and partly external to the capsule, would unite by bone, as Sir Astley Cooper had already proved the fact by experiment. As for the expediency of attempting to bring about bony union in fractures within the capsule, he would say, that it might be found hereafter, that so long a confinement as patients are now often subjected to, would not be required, if means are resorted to, by which apposition and rest can be maintained; and he thought he should be able to convince the Society, that he (Mr. Amesbury) has such in his possession; and if this should be proved by a large number of cases, there would be no doubt of the propriety of attempting to produce union by the interposition of callus, as is proved by the result of Mr. Chorley's case.

The time allotted for discussion having elapsed, the President observed, that the subject of fractures of the neck of the thigh bone would be resumed at the next meeting of the Society, when Mr. Langstaff had kindly promised to show his preparations, and he (Dr. Haslam) hoped the Gentlemen who were present as visitors, would come to the next meeting, if they should feel so disposed.

HOSPITAL REPORTS.

ST. THOMAS'S HOSPITAL.

DISEASE OF THE WOMB.—SPECIMEN OF THE TACTUS ERUDITUS OF DR. LOCOCK, LECTURER ON MIDWIFERY!

MARGARET SIMPSON, 46 years of age, a married woman, of pallid countenance and unhealthy appearance, was admitted into Elizabeth's Ward, on the 22d of February, under the care of Dr. Elliotson. The account which the patient gave of her case, at the time of admission, was as follows:—Four years ago she was seized with profuse flooding and violent pain in the region of the womb, which symptoms she attributed to a fall, received a short time previously. During the last two years she has had a copious discharge of white purulent matter, alternating with a bloody discharge; the pain felt in the womb being generally alleviated during the continuance of the latter. For the last four days she has been labouring under uterine hæmorrhage; she complains of great pain in the loins, extending down the left thigh, which is œdematous, as well as the leg. There is so much tenderness at every part of the hypogastric region, that pressure with the hand cannot be borne. There is a fœtid discharge from the vagina, with occasional bearing down pains, and difficulty in making water.

Dr. Elliotson remarked, that he had no doubt the patient was labouring under *some* chronic affection of the womb, and he requested that an examination per vaginam might be made by Dr. Locock, (the Lecturer on Midwifery at St. Thomas's, Member of the Obstetric Society, &c. &c.)

February 28. Dr. Locock's opinion, of which the following is a verbatim copy, was read by Dr. Elliotson to the gentlemen who follow his *clinique*:

"Mary Simpson.

"Several *polypoid* excrescences at the orifice of the vagina, one of the nymphæ also being in a state of enlargement, and excessively tender. *The os uteri and uterus have no morbid growth from them, but are hard and very tender to the touch, but not scirrhous.* I should think nothing could be done to remove any of the *polypous* excrescences, before the local irritation was subdued, by leeching, warm injections, &c. C. L."

Feb. 28, 1827.

There was at this time an increase of œdema in the left thigh and leg, and also the left half of the vulva. The pains were unabated, and the discharge was in greater quantity.

March 8. All the symptoms greatly aggravated; loss of appetite, tongue white, head-ache, the pulse quick and feeble. The patient is restless, appears very low, and frequently moans; she feels conscious of approaching dissolution. This event took place on the 10th, without any marked change in the symptoms having taken place.

Sectio Cadaveris.

Marasmus; left leg and thigh œdematous; skin of a yellowish tinge.

Abdomen. On opening the abdomen the great omentum was found adhering to the pelvic viscera. The uterus was much enlarged, its fundus appearing to be pushed upwards and forwards. It was partly surrounded and concealed by a very thin transparent cyst, containing about a pint of clear gelatinous serum; it was attached to the right appendage of the uterus, and floated loosely over the pelvic viscera. The lymphatic glands in the course of the iliac vessels and abdominal aorta were much diseased; some had attained the size of a hen's egg, and on cutting into them they were found to contain brain-like matter.

The cellular tissue around the iliacs abounded with deposits of a similar nature, and on each side of the anterior part of the uterus and superior portion of the vagina was seen a large mass, consisting of medullary matter, cellular tissue, and fat. It was at least two inches thick on the left side, extending from the recto-vaginal septum to the arch of the pubes and iliac vessels. On the right side, the diseased mass was not so thick, and it extended no higher than the vesico-vaginal septum. The ovaries adhered closely to the uterus; they contained, and were also connected externally with, a number of cysts of a fibrous structure, varying in size from a pea to a small pigeon's egg; some contained a pure transparent fluid; others, a fluid resembling coffee grounds; and a third set were filled with blood;—the latter were all found in the ovaries themselves.

The uterus, in consequence of the adhesion of its parietes along the median line, was divided into two parallel cavities. Its parietes were much thickened, owing to the deposit of medullary matter between its fibres; a very small portion of uterus only, at its base, presented a natural appearance; as you proceeded anteriorly, the cerebral matter seemed gradually to increase, until at length all trace of fibres became lost.

The cervix uteri and os uteri were entirely gone, and the boundaries of the space formerly occupied by them, were formed by the two masses of cancerous matter already described.

The bladder was connected at its inferior surface to the vagina and uterus by a layer of cerebriform matter. Corresponding to the extent of contact, the mucous membrane of the bladder had a soft fungous appearance, and was elevated much above the rest of the membrane, which was healthy. There was about a pint of white glairy fluid in the bladder, having very few of the characters of urine. The left ureter was distended with urine to the diameter of three quarters of an inch superiorly: the openings of both ureters into the bladder were quite pervious, although in the midst of the above described lesion of the mucous membrane. Owing to the pressure of the cancerous tumour on the left iliac vessels, the vein had its cavity obliterated at about an inch above Poupart's ligament; and there was a dark brown fibrinous coagulum extending down the interior of the femoral and internal saphenal vein, as far as the middle of the thigh. The internal membrane of each vein was dark-coloured and thickened; it had lost its natural smoothness, and adhered to the coagulum by a great number of filaments.

This case affords an excellent commentary on the text laid down by Mr. Abernethy in his lecture on the female organs of generation, published in the last number of this Journal, wherein he says—"it is scarcely possible you can fail to ascertain disease of the os internum uteri." In the case before us, however, we find a lecturer on midwifery (and a member of the Obstetric Society to boot,) failing to ascertain disease of this part. This midwife teacher writes down, that "the os uteri and uterus have no morbid growth from them,"—that "they are hard and very tender, but not scirrhous;" but what does a post-mortem examination show? why, this os uteri which has no morbid growth, and is very hard and tender, is found to be completely destroyed. It only existed therefore in the Doctor's imagination; and with respect to the *polypoid* and *polypous* excrescences of the vagina, they were merely enlarged rugæ.

So much for the "tactus eruditus" of said Dr. Locock; if the College of Surgeons in the plenitude of its wisdom shall appoint a board of examiners in the "midwifery department of medicine;" surely the acute tact of the Doctor will recommend him to the promoters of "*sound*" knowledge.

CONCUSSION OF THE BRAIN.—EXHIBITION OF OPIUM IN ORDER TO ALLAY THE SYMPATHETIC VOMITING.

The *stupifying* or *stupid* treatment adopted in the following case, shows "the stuff" that dressers are made of. It is, however, one of the many instances which come daily under our observation of the extreme ignorance manifested by the majority of the young men holding the situation of dressers. How, indeed, can it be expected to be otherwise, while the only guarantee required by the surgeons previous to holding the important office of dresser, is the payment of fifty pounds. This obtained, no matter whether the dresser be competent, although he has the care of the Hospital (during the week of his admission) four days out of seven.

Case. A stout young man was admitted into the Hospital on account of a severe contused wound on the upper part of the head, occasioned by a heavy piece of wood falling from a height upon him. When admitted, the patient was in that state of prostration, which immediately succeeds to severe injury, and bleeding was not therefore had recourse to until some degree of reaction was established, when blood was taken from the arm, and purgative medicine exhibited. In the evening of the same day the various symptoms indicative of concussion of the brain were manifest, and amongst others was occasional vomiting.

In order, as he considered, to allay this sympathetic sickness, the sapient dresser gave, *half a grain of opium, with four grains of the extract of henbane.*

On the following morning (Monday) we had an opportunity of visiting the patient with Mr. Green, when we found the man with a full and oppressed pulse, his countenance having a dull, heavy, stupid expression, and he seemed inclined to sleep. When roused he answered questions in a confused manner; he had pain in the head, the skin was hot, the tongue furred, and there was occasional vomiting.

Mr. Green, on receiving the intimation from his dresser, that a dose of opium had been given on the preceding night, evinced much surprise. He directed sixteen ounces of blood so be taken from the arm immediately, the head to be shaved, and cold lotion applied. Ten grains of calomel to be given directly, and to be followed up with house medicine until the bowels were freely relieved; the venesection to be repeated in the evening, if the pulse should be of the same kind as this morning.

These measures were found to be effectual, and the patient speedily got well.

OPERATION.

The only operation performed last week was amputation of the thigh by Mr. Travers. The operation was performed in a creditable manner.

ST. BARTHOLOMEW'S HOSPITAL.

CASE OF LEPRA ALPHOIDES.

Dr. Bateman, in his definition of this species of lepra, limits the eruption to the extremities; but as the characters of the scales and of the eruption, in the following case, agree in every other respect with the description given of the lepra alphoides by the same author, "in the small size of the patches not extending beyond the diameter of a few lines, or being confluent, and in the minuteness and greater whiteness of the scales," we have given it under that head. Indeed, the doctor himself, in speaking of the treatment of the lepra alphoides, observes that "a frequent use of the warm bath, with which a moderate degree of friction may be combined, contributes to remove the scales, and to soften the skin; or, if *the eruption be confined to the extremities*, local ablution may be sufficient," evidently meaning that it sometimes extends to other parts.

Mary Hart, æt. 10, has been the subject of this eruption for the last four months; the trunk, extremities, and hairy scalp, are studded with small whitish circular scales. No uneasy sensations attend it, except a slight occasional itching when warm in bed. Many remedies had been employed previous to her admission into the hospital, but without benefit.

Mr. Lawrence ordered a warm bath three times a week, five grains of the hydr. c. creta every second night, and a dose of rhubard on the following morning. This plan was followed up for some time without any apparent advantage, until, at length, the child became salivated. From that period the scales began to decline; in the course of a week, the extremities were almost free; and in three from the occurrence of salivation, the child left the hospital free from the unseemly disease. We have considered this case worth reporting, from the circumstance that mercury, *carried to the extent of salivation*, seems to have effected the cure. Various means had been previously tried, and mercury itself, for some time, without the removal of the affection; but as soon as the mouth became affected, it began to die away, and progressively continued to do so until the cure was completed.

TUMOUR NEAR THE ANGLE OF THE JAW.

Mary Bray, æt. 34. This patient, a thin woman, of ruddy complexion and healthy appearance, states, that between seven and eight years ago, she caught cold from sleeping in a damp bed, and that a sore throat ensued. Soon after she perceived a small tumour situated immediately behind the angle of the jaw, which increased very slowly for four years, but, latterly, has grown more rapidly. She has had good health for many years, and lived abstemiously. She has now a swelling immediately behind the angle and ramus of the jaw, considerably larger than a hen's egg. It is oval, hard, and irregular on the surface, as if composed of four unequal lobes, and easily moveable both on the subjacent parts, and under the skin, to which it is loosely attached. It is not, nor has it ever been painful, or produced the slightest inconvenience in any respect.

The tumour was removed by Mr. Lawrence by a single incision of about five inches, carried over its long axis, being connected only by loose cellular tissue, and quickly dissected out. Profuse bleeding took place from several arteries during the excision, which was not interrupted on that account; faintness came on, and stopped it. A small slice of parotid gland was found at the base of the tumour. The surface of the latter was tuberculated, and covered by a thin closely-adhering whitish capsule. Its substance was firm and compact, approaching, in colour and appearance, to the softer form of scirrhus. Its cut surface had a slightly lobulated appearance, and of uniform texture throughout. The flap was laid down and slightly approximated by adhesive straps, subsequently removed, the wound being simply covered by a damp cloth. A considerable portion of the incision united by adhesion; but a copious flow of saliva took place from its centre a few days after the operation, and continued for about ten days. She was discharged cured on the 22d of February.

ERRATUM.

Page 810, 2d column, 5th line from bottom, for "Hermite in Ecosse," read "Hermite *en* Ecosse."

Correspondents in our next.

END OF VOL. XI.